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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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Of Foreign Works, H. Beaupoil; Cit. Berthollet; Fourcroy; Vauquelin; Collet Descotils; Brugnatelli; Professor Lichtenberg; Ritter; Hauy; J. Drapernaud; Klaproth; Van Marum; J C. Bartholdi; Jerome De Lalande; J. L. Guy Lussac; Dr. P. A. Nemnich.

And of English Memoirs, abridged or extracted; R. Ramsden Bradley, Esq.; Mr. Robert Green; Rev. Edmund Cartwright; Dr. John Winterbottom; R. B; Benjamin Smith Barton, M. D.; Mr. Edward Massey; Mr. David Charles; Smithson Tennant, Esq. F. R. S.

Of the Engravings the Subjects are; 1. Machine of conſiderable Power for clearing Roads of Mud, by Dr. Winterbottom. 2. Mr. Robert Green's Hand Drill for P[illegible]s. 3. The Rev. E. Cartwright's Three-Furrow Plough. 4. Diagram for inveſtigating the Figure of the Earth. 5. New Galvanic vanio-Apparatus, convertible at Pleasure into one or more Plates. 6. Hydraulic Machine operating by the rotation of two Pinions, in a Water Veſſel. 7. An improved Jib for a Crane, by Mr. Bramah. 8. New Filtering Apparatus, by Messrs Harman and Dearn. 9. Galvanic Apparatus. 10. An ancient Lock uſed in Egypt and Western Asia for above three thousand Years. 11. Developement of the Mechaniſm of the Lock. 12. Sketch of the Orbits of the New Planets, by Jerome de Lalande. 13. Cryſtals of Arseniated Copper, by Hauy. 14. Telegraph by the Human Figure. 15. Machine for levelling the Surface of Land, by Mr. David Charles. 16. Shaded Sections of a Clock, which strikes the Hours by simpler Mechanism, and with greater regularity than usual, by means of a Pendulum substituted in the Place of a Fly. 17. Specimen of a very curious antique Composition or Painting in coloured Glass, by an Art at present lost. 18. Improved Lamp for producing a strong Heat in Chemical Experiments, by Mr. Accum.

Soho Square, Auguſt, 1804.

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THE ARTS.

MAY, 1804.

ARTICLE I.

Letter from C. WILKINSON, *Eſq. on the Means of ſimplifying and improving the Galvanic Apparatus.*

To Mr. NICHOLSON.

SIR,

THE communication from your ingenious correſpondent I. R. I. afforded me conſiderable pleaſure. As you favoured me with his idea how the galvanic power may be increaſed to an immenſe degree, prior to its appearance in your valuable Journal, I have been induced to reflect upon various modes of galvanic arrangements. Introduction.

If the whole apparatus be made to conſiſt only of a ſingle plate reflected backwards and forwards, ſo as to expoſe an immenſe ſurface, no plate of copper would be required, only taking care, which in this inſtance would be eaſily effected, that one ſide of the plate only ſhould be expoſed to the action of the acid. For, from various experiments, I am perſuaded that the other metal anſwers no other electrical purpoſe, than to guard and protect the zinc ſide, to which it is ſoldered from being acted upon by the acid. Even cement anſwers very well, if we only preſerve through the cement a good conducting medium to the zinc. Thus with a zinc plate of eight inches diameter, if only a piece of copper the ſize of a half-

In apparatus of one ſingle plate, the coating of copper is unneceſſary.

Cement will do as well.

penny be ſoldered in its centre, and all the reſt of that ſurface, which is uſually ſoldered to copper or ſilver, be well covered with cement, equal effects would be obtained.

Proof in the Couronne de Taſſes;

In a Couronne de Taſſes I find the ſame effects are produced, whether the copper be of a correſponding ſize to the zinc plates, or, whether they be merely ſimple copper or ſilver wires.

and in a pile.

Upon this principle I have conſtructed a pile, and find it produces the ſame effects, as if the whole ſurface were covered with copper; I purpoſe ſoon to arrange a trough upon the ſame principles, and I am perſuaded, that the more tedious and expenſive part of galvanic apparatus may thus be prevented.

The corroſion in a trough is greateſt near the air.

When a trough has been ſometime employed, upon removing the plates, the loſs of metal, I always obſerve, does not take place uniformly over the ſurface, but in the upper part, which in the galvanic action is the moſt expoſed to the atmoſphere, is the moſt acted on, and towards the bottom, the metal is very little altered. In order to preſerve a more equal action, I am now preparing a battery formed of plates of ten inches by two and a half, the longeſt ſide placed horizontally, and I am perſuaded, that this ſized plate will produce more active effects than a plate of five inches ſquare.

Hence a *pile of diſhes* would be preferable.

It is well known, that in every galvanic operation, oxidation is produced, and if oxigen can be procured from the ſurrounding air, the effect would be more eaſy than producing it from the decompoſition of water. I have no doubt, that if a ſeries of zinc plates formed like large pewter diſhes, were to be arranged in a pile-like form, inſulated from each other, and the galvanic mixture to be poured in the hollow part; the lower ſide being covered with cement, excepting that from the centre a piece of copper ſhould be ſoldered, and ſo projecting as to be in contact with the fluid in the cavity of the plate below; that a very powerful apparatus would thus eaſily be formed.

On the ſubſtitution of air inſtead of wet mediums by Dyckhoff.

In the latter part of your Journal I peruſed with a conſiderable degree of ſurpriſe, ſome obſervations of a Mr. Dyckhoff relative to the ſubſtitutions of thin ſtrata of air, inſtead of wet media. As the reſults of his experiments appeared ſo contrary in principle to any I had tried, I immediately repeated them in the manner he has deſcribed, with interpoſed lentils of glaſs, ſo as to render the ſeparation very ſmall. I then arranged ſeveral ſeries of plates of one inch, to plates of ſeven inches in diameter; becauſe he has not particularized the ſize of the plates he employed. I arranged piles of theſe different ſizes to the number of twenty pairs in each arrangement, and obſerved not the ſlighteſt galvanic effect, either by my tongue or by any other teſt. I employed a very excellent condenſer; no influence at all was evinced; and, laſtly, I ſubjected it to the moſt delicate electroſcope we have, viz. the muſcular fibres of a frog. Not the ſlighteſt diſturbance took place, although I fancy I have demonſtrated, in the elements of galvaniſm I have publiſhed, that the ſenſibility of this animal electroſcope is fifty thouſand times greater than that of the condenſer.

The experiment did not ſucceed.

As to the charging of a Leyden phial, I am convinced it is perfectly erroneous to ſuppoſe it has ever been done. I have employed from fifty pair of plates to fifteen hundred, and never yet have produced any charge. Nor indeed could ſuch be expected from the weak intenſity of the ſtate of electricity in galvanic operations; for a jar cannot be charged until a ſufficient quantum is accumulated to overcome the reſiſtance of the ſurrounding air, ſo abſolutely requiſite to the charging of a Leyden phial.

Conviction of the author that galvaniſm cannot charge a Leyden jar.

I am, Sir,

Your's, &c.

C. WILKINSON.

ANNOTATION. W. N.

THE valuable obſervations in the preceding letter, will naturally ſuggeſt improvements to thoſe who are employed in the conſtruction of galvanic apparatus. It muſt be a great advantage, that the expence of copper and the work of ſoldering, or placing it, will be almoſt entirely ſaved. *Fig.* 1. *Plate* III. ſhews a ſimple method of diſpoſing a ſingle plate of zinc of large ſurface in a trough. It is ſuppoſed to have been made ſufficiently hot to bed itſelf in cement at the bottom of the box in which it is placed, and its two ends A and B are ſecured in the ſame way. The ſhaded ſpaces repreſent the cavity occupied by acid, and the dotted ſpace is left empty.

Form of galvanic apparatus in a ſingle piece.

A communication may be made by a copper wire from the wet or corroded ſurface to the oppoſite or dry ſurface, as ſhewn at A and C.

Another of many pieces acting as one.

As it may be difficult to bend a very long piece of laminated zinc, and to keep it flat, ſo as to give it a ſecure and cloſe fixture in the cement, I ſhould prefer a number of ſtrait pieces ſcrewed together with varniſhed pieces of wood between their extremities: every ſecond piece being perforated on the acid ſide, to permit a free communication.

More particular account of the pile of diſhes.

Mr. Wilkinſon's pile of diſhes promiſes to be very effectual, and of eaſy conſtruction. I ſuppoſe the diſhes to be of zinc, either caſt in a metallic mould or ſtamped, and that each ſhould be provided with three ſhort copper legs, ſoft-ſoldered on; after which, the lower face of the diſh ſhould be well defended by varniſh or cement, and alſo the lower ends, but not the ſides of the copper legs. Thus prepared, they might be eaſily builded up, and charged by a proper funnel, and an appropriate meaſure for dealing out the acid.

Galvanic trough of one metal convertible into a ſingle plate, by varying the commu

In *Fig.* 2. I have ventured to offer the ſketch of a trough, conſiſting of zinc plates only, principally becauſe it appears capable of being as ſpeedily charged with acid as another trough, and may with facility and at pleaſure be uſed either as one ſingle plate, or as the uſual ſeries of plates, acting in ſucceſſion upon each other. Let A B repreſent a trough conſtructed after the manner of Cruickſhank; excepting that the plates are of zinc only: and let the alternate ſhaded cells be charged with diluted acid, while the dotted cells continue empty. This may, without difficulty, be done at one pouring, by means of a channel in the wooden ſide of the trough.

Arrangement to produce the uſual effect of a large plate.

Whether this trough, ſo charged, ſhall act as one extended galvanic ſurface, or as a continued ſeries, will be governed by the copper wires of communication. If the former, then a long copper wire having branches deſcending from it into every one of the acid cells muſt be duly placed; and another wire parallel to, but not in contact with, the former, and having double branches deſcending into all the empty cells, ſo as to touch both the dry metallic ſurfaces, muſt alſo be placed. Whenever a communication is made between theſe two principal wires or conductors, the galvanic energy will paſs through the medium of communication. I have not given a diagram, becauſe this diſpoſition is very obvious.

If the latter effect be required, namely, that the trough ſhould act like a continued ſeries, the connection of the troughs muſt be differently made. A number of clipping pieces or ſpring forks muſt be provided of copper wire, as repreſented by the curved lines *a. b, c, d,* &c. from each of which proceeds a third leg or branch, as ſeen in the figure. Theſe are applied (moſt conveniently above, but) not ſo as to touch each other; the clipping part of each embracing the dry ſurfaces of a pair of the zinc pieces, which are acted upon by the ſame maſs of acid, while the third leg is immerſed in the acid of the next adjacent cell. Every one of their legs or branches is diſpoſed towards the ſame region or part of ſpace; by which means the acid of each compartment acting upon a pair of zinc plates, on one ſurface only, is made to communicate, by the interpoſition of copper, with the uncorroded ſide of the next pair in ſucceſſion; and ſo on, exactly as in the common trough or pile.

Arrangement for the effect of a ſucceſſion of plates.

II.

Obſervations and Communications on the Dry Rot in Timber, made to the Society for the Encouragement of Arts.

(Concluded from p. 318 of Vol. VII.)

Second Letter from Benjamin Johnson, *Eſq.*

SIR,

THE obſervations I ſent yeſterday were taken from different parts of my note-book, in haſte, becauſe the ſecond Tueſday in December was paſt; for it was by accident I ſaw the advertiſement on Saturday; but wiſhing not to be deficient in information, I trouble you again.

The leaves of the plant appearing exhauſted and dead, is owing to their having imparted all their juices to the wood, which changed it to a fungus, and not to a powder, like rottenneſs from length of time.

A more full account of the plant that occaſions the dry rot;

The *Boletus Lachrymans* is of the fungus tribe, and is one of the few that have leaves, as the miſeltoe, &c.

Nothing is more eaſy than to prevent the damage from the plant. Beſides what I ſaid yeſterday, I am poſitive that a tile laid cloſe along the walls round the room, would prevent the growth of the plant, even without mortar; and perhaps it is only neceſſary where the walls are next to the air.

and the method of cure.

Charring the ends of the joiſts for a few inches, and charring the ſide of the wainſcot at bottom, next to the walls would be ſufficient; for the plant cannot adhere to any thing but wood, and that poſſeſſed of its natural juices, to a certain degree; ſo that I queſtion if old dry oak would receive it.

All the white ſoft woods, as beech, poplars and deals, are for a long time ready to receive it. Repairing the damage with freſh wood, without removing the earth and plant, is only feeding the evil.

The plant is of the creeping kind, and cannot riſe two inches; ſo that wood in all caſes, muſt be in contact with the earth to ſupport it.

A fungus broader than the palm of one's hand, and an inch or more in thickneſs, is commonly ſeen at the bottom of an old poſt, on the ſurface of the earth; but it is not eaſy to diſcern whether the wood or the earth furniſhes the matter; ſo true is the obſervation of Muller:—"*Dans l'étude de la nature, on peut nous comparer à de petits enfans qui commencent à ouvrir les yeux; nous voulons parler beaucoup, et nous ne faiſons que bégayer.*"

I am, Sir,
Your moſt obedient Servant,
BENJAMIN JOHNSON.

Ipſwich, Dec. 21, 1799.
To the Secretary.

N. B. The qualities of this plant are unknown to moſt Engliſh botaniſts, as appears from their publications; but they are known to the Germans, who have habitually uſed more wood in their buildings than we have.

Third Letter from the Same.

SIR,

Aſſured that the purſuits of the Society for the Encouragement of Arts, &c. aim at the full inveſtigation of whatever they propoſe for the public benefit, I cannot perſuade myſelf that I am troubleſome in going a little further into this ſubject.

I had lately a converſation with an old friend, who ſhowed me two parcels of rotten wood, from an oak barn floor, laid about ſixteen years ago. After lying twelve years it ſhook upon the joiſts. On examination, it was found to be rotted in various parts, and the planks, two inches and a half in thickneſs, were nearly eaten through, though the outſide was gloſſy, and without blemiſh. The joiſts, and a large middle beam were laid at the ends, in brick and mortar, to create a firm level. No earth was near the wood; and he thinks that no air could find a paſſage. The rottenneſs was partly an impalpable powder, of the colour of Spaniſh ſnuff, and other parts were black, as if burnt; the reſt was clearly a fungus.

Some account of a barn floor decayed.

This gentleman is a perſon of undoubted veracity; but a nice and exact obſervation is neceſſary in ſuch examinations. He thought nothing of any plant, and it is likely there was none of the Boletus; ſo that my aſſertion that it was always to be found, was rather too ſyſtematic.

It does not appear to have been cauſed by any plant.

I aſked him if the timber was dry when laid down. He could not however ſay that had been particularly adverted to. It had been ſawed from a large oak, and was, as he thought, in all reſpects proper for a barn floor. As this ſeems not the operation of the Boletus, how did it happen?

We know that the oak, when in vegetation, is ſubject to what I ſhall call an exudation of juices, which produces the fungus, named the Agaric of the oak, with which the Druids of old played many tricks. The oak, then, if ſawed into thick quantities, may emit theſe ſame juices, as the progreſſive courſe of nature to its entire decay.

On the decay of oak timber.

We have all ſeen oaks of vaſt ſize and ancient record, with a great part of the outſide whole, and all the inſide gone; perhaps the work of a century. In all hollow trees fungus is diſcoverable. To uſe a law term, it is a *miſnomer* to call it dry-rot; for the rotting principle is in moiſture.

I had never ſeen the rot upon ſo large a ſcale as in timber, till lately. The prevention, then, of beams, rafters, large joiſts, and poſts, put into the earth, from decay by the rot, is in charring only, which will dry up all the fungus juices of wood in large ſubſtance. Paint, or a bituminous preparation, may probably ſtop up the pores, and prevent the rot in ſlight work, where the treatment I before obſerved, with fire, might be incommodious, as in half-inch wainſcot, &c.*

The preparation of wood for preſerving it, ſhould be either charring, or bituminous paint.

The

Durability of charcoal.

The incorruptibility of charcoal is atteſted by undoubted hiſtorical facts, at the deſtruction of the famous temple at Epheſus. It was found to have been erected on piles that had been charred; and the charcoal in Herculaneum, after almoſt 2000 years, was entire and undiminiſhed.

I am, Sir,
Your moſt obedient Servant,
BENJAMIN JOHNSON.

Ipſwich, December 26.

Letter from RICHARD RAMSDEN BRAMLEY, *Eſq. of Leeds, relative to the Dry Rot in Timber.*

To CHARLES TAYLOR, Eſq.

SIR,

Introduction.

I take the liberty of incloſing to your care an Eſſay on the Dry Rot in Timber, which you will be ſo obliging as to lay before the Society for the Encouragement of Arts, &c. Should this Eſſay be deemed worthy of attention, or ſhould any farther notice be neceſſary reſpecting it, every information that may promote the views of your reſpectable Society will be given with pleaſure by,

Sir,
Your moſt obedient Servant,
R. RAMSDEN BRAMLEY.

Leeds, Aug. 26, 1799.

As the Society for the Encouragement of Arts, &c. have for ſome years offered a premium for the diſcovery of the cauſe occaſioning the dry rot in timber, of which, it ſeems, no ſatisfactory account has yet been received; ſhould the following prove ſo, it will give the author much pleaſure. To bring the matter to the teſt by experiments, would require the obſervation of a long period, and in ſelected ſituations.

Wood, uſed for the general purpoſes of man, is cut down at different periods; and although it may be felled at the proper ſeaſon, or when moſt free from ſap or moiſture, it is not always to be effected.

Even admitting it to have been cut down in the moſt favourable ſituation, it ſtill abounds with ſuch an extra proportion of

Facts and obſervations concerning the dry rot in timber, and its cure.

moiſture, as to require a regular expoſure to the air, prior to its being applied to uſe, if we wiſh to guard againſt that ſhrinking which always takes place, where this precaution has not been taken.

Although the fir kind contains leſs of this watery portion, yet it aſſuredly poſſeſſes a conſiderable ſhare; and it is in this ſpecies, I apprehend, that the evil called the dry rot moſt generally occurs, as from the facility of working the ſame, it is moſt generally applied in buildings.

But ſuppoſing it to be fir, or any other ſpecies; wood felled when abounding with any extra proportion of ſap, and applied to uſe without the proper ſeaſoning or expoſure to a free current of air, until ſuch extra moiſture as has had time to exhale, is moſt liable to the diſeaſe in queſtion; and the cure, or principal prevention againſt it, would be the precaution of felling all wood *only* at the proper ſeaſon, or when the ſap is not in circulation. The next mode of prevention would be to uſe ſuch wood only as has been for a conſiderable period expoſed to the influence of a free current of air, or where convenience will admit, to that of air heated to a moderate degree; ſuch air extracting with greater facility the incloſed moiſture, and in a more certain ratio than the irregularity of our atmoſphere will allow.

In all rapidly-improving countries, this evil is likely to be an increaſing one, as the current demand for wood generally exceeds the ſupplies laid by in ſtore, ſo as to be applied to uſe in regular ſucceſſion, after being properly ſeaſoned.

Another cauſe that affects all wood moſt materially, when not fully dried, is the application of paint, the nature of which prevents all exhalation, and confines the incloſed moiſture, till it occaſions a fermentation through the whole fibrous ſyſtem of the wood, and brings on a premature ſtate of decompoſition, or the dry rot.

A ſimilar evil may be induced, in conſequence of any newly-finiſhed building having all the doors and windows ſhut up, and that for ſome length of time, particularly in moiſt weather. The wood, even though unpainted, is thus frequently placed in an atmoſphere more charged with vapour than its own internal contents, and is conſequently in an imbibing inſtead of an exhaling ſtate, and tending to decay. Wood placed in dampiſh ſituations, and the ends of timbers near to moiſt walls, ſuffer from ſimilar cauſes.

Facts and obſervations concerning the dry rot in timber, and its cure.

What particularly attracted my obſervation to the circumſtances was this, that both oak and fir poſts were brought into this premature ſtate of decay, from their having been painted prior to the due evaporation of their moiſture; and then extending the obſervation, and tracing the hiſtory of other wood affected in a ſimilar manner, I am convinced that the evil frequently thus originates, and its prevention would be in uſing timber, previouſly well dried and ſeaſoned.

RICHARD RAMSDEN BRAMLEY.

SIR,

A conſiderable time has elapſed ſince I furniſhed you with ſome obſervations relative to the dry rot in timber, and having been ſince engaged buſily in draining from 4 to 5000 acres of ground, further ideas on the ſubject of the dry rot have in the interim recurred to me from the work I have been engaged in, which, if the reſpectable Society to which you are Secretary think worthy attention, they may add to, or connect with my former ideas, as may be deemed moſt uſeful. Where houſes are troubled with damp walls, near to the earth's ſurface, it is generally, if not univerſally, occaſioned by the percolation of water from the higher adjoining ground, which, thus intercepted in its current, attempts to follow the general hydroſtatic law, of elevating itſelf, by the ſyphon line, to a height equal to that from whence it has its origin. Thus, in houſes differently ſituated, we ſee the damp ariſing, to varying degrees of height, on the walls; and thoſe are probably all correſponding to the height at which the moiſture circulates in the adjoining ground. At its firſt entrance to the building, and whilſt the moiſture is in ſmall quantity, the excavated part of the foundation wall may abſorb, and gradually quit ſuch proportion; but the exceſs, as is generally the caſe in moiſt weather, exceeding that power, the foundation ſtones are then ſaturated in a more rapid proportion than the adjoining rarified internal atmoſphere can evaporate: the watery particles then creep up, in degrees proportionate to the aſcent from which they originally deſcended, excepting when prevented, or driven off by the ſuperior heat of the adjoining rooms, when, in addition to the diſagreeable damp they cauſe, they frequently occaſion conſiderable damage to pictures, furniture, &c. Drains laid out athwart the aſcending ground, with a

very

Facts and obſervations concerning the dry rot in timber, and its cure.

very ſlight deſcent or fall, and made of the depth of one yard for each yard of aſcent, and from the foundation until equal to the height that ſuch damp ever riſes, would, there is little doubt, completely ſecure the houſe and furniture from the inconveniences hitherto ſuſtained, and would generally prove an effectual prevention to moſt caſes of the dry rot, where it originates in extreme moiſture. I am of opinion that the fungus which pervades decaying wood is not the firſt cauſe, but an attendant on the peculiar ſtate to which ſuch wood has been reduced by prior cauſes. The diſſeminated ſeeds finding a proper bed, or *nidus*, like the muſhroom, toad-ſtool, &c. fix there their abode, and pervade the whole ſubſtance, thus accelerating the general law of Providence, which tends to make all matter re-productive.

Cellars, or ſuch other places, ſhould be drained in the manner I have above mentioned, by taking off the percolating water, prior to its gaining admiſſion to or contact with the walls; and it is probable that, in moſt caſes, a ſingle drain will have complete effect; it would aſſuredly do ſo, if it was not for the variation of the earth's internal ſtrata, which are not eaſily diſcernible. If attention to this rule was paid prior to the building any new ſtreets in towns, it would prove eſſentially uſeful.

I am, with eſteem,
DEAR SIR,
Your's truly,
Leeds, June, 1803. R. RAMSDEN BRAMLEY.
To Mr. Charles Taylor.

The Society have been informed, that mortar made of lime from burnt chalk is much more deſtructive to timber than ſtone lime, or that burnt from lime-ſtone. Chalk lime attracts moiſture; and communicating it to any timber which it touches, occaſions its decay.

Sea ſand is alſo prejudicial, if made into mortar, from a ſimilar quality of attracting moiſture from the atmoſphere: this may in ſome degree be corrected by waſhing the ſand well in freſh water, where good ſand cannot be procured.

Good mortar, where any is required to be in contact with timber, may be made from a mixture of ſtone lime freſh burnt, and river ſand, to which a very ſmall quantity of common brown, or yellow iron ochre, ſhould be added, and well incorporated therewith.

III.

On the Figure of the Earth. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

IN ſome of your late Journals I obſerve a paper on the figure of the earth, by Mr. John Playfair, profeſſor of mathematics in the Univerſity of Edinburgh, containing ſeveral new theorems, and ingenious remarks, on a ſubject which has engaged the attention of the firſt mathematicians of Europe ſince the days of Newton. On reading it, I was led to examine the properties of ſpheroidal triangles, and to inveſtigate the problem, propoſed by the author, for determining the dimenſions of the earth from the length of the ſtraight line or chord joining two places whoſe geographical ſituations are given. Theſe are intended to form the principal ſubject of this letter; but, before I proceed to them, I beg leave to make a few obſervations on that paper, without any view to cavil, or detract from its real merits.

Obſervations upon Profeſſor Playfair's memoir on the figure of the earth.

After taking notice of the diſagreement in the compreſſions of the terreſtrial ſpheroid, which reſult from the compariſon of different meaſurements, he aſſigns, as the principal reaſon for this inconſiſtency, the local irregularities in the direction of gravity, ariſing in ſome ſituations from the attraction of mountains, and in others from the unequal denſity of the materials under, and not far from, the ſurface of the earth. That the firſt has a ſenſible effect on the plumb-line has been proved by accurate and undeniable experiments; the ſecond is an ingenious and probable conjecture, which the ſurveys carrying on in Great Britain and France will afford data to refute or confirm. But though the former may operate in the general ſurvey of a country, where the obſerver has not his choice of ground, it has always been avoided as much as poſſible in meaſurements made for the expreſs purpoſe of determining the figure of the earth; and though the latter may produce ſome perceptible difference in obſervations made in nearly the ſame latitudes, does the author think it ſufficient to account for the great diſagreement in the reſults from the compariſon of diſtant obſervations? Is it not much more probable, without giving up the elliptic figure, that ſome of the obſervers may have uſed different ſtandard meaſures from the reſt, or not made proper allowances for the alteration of their lengths in different temperatures? In ſhort, this circumſtance appears to me ſufficient to account for ſome ſmall local irregularities, but wholly inadequate to explain the great differences in the general reſults.

Obſervations upon Profeſſor Playfair's memoir on the figure of the earth.

The author then proceeds to point out ſeveral methods of calculating the dimenſions of the earth from terreſtrial meaſurements. The firſt applies to the caſe where two arches of the meridian are given in different latitudes, which, under the moſt favourable circumſtances, is incomparably the moſt accurate that can be employed. The rules he gives are certainly very ſimple, and in ſome reſpects new; but he ſeems to be miſtaken when he aſſerts, that the calculation muſt be made by rules quite different from thoſe that have been hitherto given. Euler's * is eſſentially the ſame with his own; and Du Séjour, Legendre, Delambre, &c. have given many accurate theorems, which may be applied to this purpoſe. The ſecond method is, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the ſame latitude; and the third from the meaſures of degrees of the curve perpendicular to the meridian in different latitudes. His theorems for both are very accurate and ſimple. But the principal novelty of Mr. Playfair's paper is, the method he propoſes of finding the figure of the earth from the length of a ſtraight line or chord joining two places whoſe geographical ſituations are given. As he has left the ſolution of this problem to ſome future occaſion, the following perhaps may not be unacceptable :

Let PAO (*Plate* III. *Fig.* 1.) repreſent one quarter of the ellipſis, by the revolution of which round the ſemi-conjugate axis PC, half the terreſtrial ſpheroid is generated. Let C be the center of the earth, P the pole, CO the radius of the equator $=a$, CP half the polar axis $=b$, and $c=$ the compreſſion at the poles, or the exceſs of a above b. Let A in the

Solution of the problem for finding the figure of the earth, from the length of a chord joining two known places, &c.

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* Memoires de L'Academie Royale des Sciences Belles Lettres a Berlin, 1753.

meridian

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

meridian PAO be one of the extremities of the meaſured chord; and B in the meridian PB the other extremity; let AD, BF be drawn perpendicular to CP, BE perpendicular to the plane CPAO, and let AB, AE, FE be joined. Then will AE^2 be $=(CD-CF)^2+(AD-FE)^2 = CD^2+AD^2 \times CF^2+FE^2-2\,CD\times CF-2\,AD\times FE$, and $AB^2=AE^2+BE^2=CA^2+CB^2-2\,CD\times CF-2\,AD\times FE$.

Now let λ, φ be the latitudes of A and B expreſſed in decimals of the radius 1, ω the difference of longitude or the angle BFE, and D the length of the meaſured chord $=$ AB. Then from the properties of the ellipſis we have

$$CA^2=\frac{a^2\ \text{coſ.}\ \lambda^2+b^2\ \text{ſin.}\ \lambda^2}{a^2\ \text{coſ.}\ \lambda^2+b^2\ \text{ſin.}\ \lambda^2}=a^2-2\,ac\ \text{Sin}\ \lambda^2$$

$$AD=\frac{a^2\ \text{coſ.}\ \lambda}{\sqrt{(a^2\ \text{coſ.}\ \lambda^2+b^2\ \text{ſin.}\ \lambda^2)}}=a\ \text{coſ.}\ \lambda+c\ \text{coſ.}\ \lambda\ \text{ſin.}\ \lambda^2$$

and $CD=\dfrac{b^2\ \text{ſin.}\ \lambda}{\sqrt{(a^2\ \text{coſ.}\ \lambda^2+b^2\ \text{ſin.}\ \lambda^2)}}=a\ \text{ſin.}\ \lambda-c\ \text{ſin.}\ \lambda$ $(2-\text{ſin.}\ \lambda^2)$, neglecting the powers of c higher than the firſt, becauſe c is very ſmall in compariſon of a. Whence by ſubſtitution, and putting $\delta^2=2\,a^2\ (1-\text{ſin.}\ \varphi\ \text{ſin.}\ \lambda-\text{coſ.}\ \varphi\ \text{coſ.}\ \lambda\ \text{coſ.}\ \omega)$, we obtain the following equation;

$$\delta^2-c\left(4\,a\,(\text{ſin.}\ \lambda-\text{ſin.}\ \varphi)^2-\frac{\delta^2}{a}\,(\text{ſin.}\ \lambda^2+\text{ſin.}\ \varphi^2)\right)=D^2,$$

and by extracting the ſquare root of each ſide, and rejecting the ſquare, cube, &c. of c, there reſults,

$$\delta-c\left\{\frac{2\,a}{\delta}\,(\text{ſin.}\ \lambda-\text{ſin.}\ \varphi)^2-\frac{\delta}{2\,a}\,(\text{ſin.}\ \lambda^2+\text{ſin.}\ \varphi^2)\right\}=D,$$

$$\text{or}\ a+c\left\{\tfrac{1}{2}\,(\text{ſin.}\ \lambda^2+\text{ſin.}\ \varphi^2)-\frac{2\,a^2}{\delta^2}\,(\text{ſin.}\ \lambda-\text{ſin.}\ \varphi)^2\right\}=\frac{D\,a}{\delta}$$

This equation may be otherwiſe expreſſed thus; let a ſpherical triangle be conſtructed, having two ſides equal to the polar diſtances of A, B, and contained angle $=$ their difference of longitude; whence find the third ſide, which put $=\vartheta$. Then will $\text{ſin.}\ \varphi\ \text{ſin.}\ \lambda+\text{coſ.}\ \varphi\ \text{coſ.}\ \lambda\times\text{coſ.}\ \omega=\text{coſ.}\ \vartheta$, and $1-\text{ſin.}\ \varphi\ \text{ſin.}\ \lambda-\text{coſ.}\ \varphi\ \text{coſ.}\ \lambda\ \text{coſ.}\ \omega=1-\text{coſ.}\ \vartheta=2\ \text{ſin.}\ \tfrac{1}{2}\vartheta^2$; therefore $\delta=2\,a\ \text{ſin.}\ \tfrac{1}{2}\vartheta$, and $D=2\,a\ \text{ſin.}\ \tfrac{1}{2}\vartheta+c\left\{\text{ſin.}\ \tfrac{1}{2}\vartheta\ (\text{ſin.}\ \lambda^2\times\text{ſin.}\ \varphi^2)-\dfrac{(\text{ſin.}\ \lambda-\text{ſin.}\ \varphi)^2}{\text{ſin.}\ \tfrac{1}{2}\vartheta}\right\}$. — The value of δ is manifeſtly equal to the length of a ſtraight line joining two places, whoſe latitudes are λ, φ, and difference of longitude ω, on a ſphere, whoſe radius is a.

From

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

From this equation the following method of determining the figure of the earth is deduced. Let l be the length of a meaſured chord, and λ, ϕ, ω the latitudes and difference of longitude of its extremities; find ϑ as above, and let $m = 2\,\text{ſin.}\,\frac{1}{2}\vartheta$, and $n = \text{ſin.}\,\frac{1}{2}\vartheta\,(\text{ſin.}\,\lambda^2 + \text{ſin.}\,\phi^2) - \frac{(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)^2}{\text{ſin.}\,\frac{1}{2}\vartheta}$. Then if we reject all the powers of c higher than the firſt, we ſhall have the ſimple equation $ma + nc = l$. In like manner find a ſimilar equation $m'a + n'c = l'$, correſponding to any other chord whoſe length is l', and there will reſult $a = \frac{n'l - nl'}{mn' - m'n}$, and $c = \frac{m'l - ml'}{mn' - m'n}$. The approximation may be eaſily carried further by including the ſecond power of c, and thus finding an equation of the form $ma + nc + pc^2 = l$; but this labour would be uſeleſs, as the method itſelf does not admit of greater accuracy. If $\phi = \lambda$ the equation becomes $\frac{D}{2\,\text{ſin.}\,\frac{1}{2}\vartheta} = a + c\,\text{ſin.}\,\phi^2$, as is found by Mr. Playfair in § 31.

From the firſt equation a rule may be eaſily derived for calculating the difference of longitude of two places, when their latitudes and diſtance are given. For by tranſpoſition we have

$$\delta^2\left(1 + \frac{c}{a}(\text{ſin.}\,\lambda^2 + \text{ſin.}\,\phi^2\right) = D^2 + 4ac\,(\text{ſin}\,\lambda - \text{ſin}\,\phi)^2,$$

and by diviſion, and rejecting the powers of c higher than the firſt $\delta^2 = D^2 + \frac{c}{a}\left(4a^2\,(\text{ſin}\,\lambda - \text{ſin.}\,\phi)^2 - D^2\,(\text{ſin.}\,\lambda^2 + \text{ſin.}\,\phi^2)\right)$: but δ^2 is $= 2a^2\,(1 - \text{ſin.}\,\lambda\,\text{ſin.}\,\phi - \text{coſ.}\,\lambda\,\text{coſ.}\,\phi\,\text{coſ.}\,\omega)$, therefore $\text{coſ.}\,\omega = \frac{1 - \frac{D^2}{2a^2} - \text{ſin.}\,\lambda\,\text{ſin.}\,\phi}{\text{coſ.}\,\lambda\,\text{coſ.}\,\phi} - \frac{c}{a}\left(2\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)^2 - \frac{D^2}{2a^2}\,(\text{ſin.}\,\lambda^2 + \text{ſin.}\,\phi^2)\right)$ and putting $\frac{1 - \frac{D^2}{2a^2} - \text{ſin.}\,\lambda\,\text{ſin.}\,\phi}{\text{coſ.}\,\lambda\,\text{coſ.}\,\phi} = \text{coſ.}\,\omega'$ we have $= \omega' + \frac{c}{a} \times \frac{2\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)^2 - \frac{D^2}{a^2}}{\text{ſin.}\,\omega'}$ $\overline{(\text{ſin}\,\lambda^2 + \text{ſin.}\,\phi^2)}$, which rule may be thus expreſſed. Let there be a ſpherical triangle, having two of its ſides equal to the polar diſtances of the places, and the third ſide d ſuch that ſin.

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$\text{ſin.}\ \frac{1}{2} d = \frac{D}{2a}$: find the angle ω' contained between the polar diſtances, and the difference of longitude ω will be $= \omega' + \frac{2c}{a} \times \frac{\text{coſ.}\ \frac{1}{2} d^2\ (\text{ſin.}\ \lambda^2 + \text{ſin.}\ \phi^2) - 2\ \text{ſin.}\ \lambda\ \text{ſin}\ \phi}{\text{ſin.}\ \omega'}$.

The latitude ϕ may alſo be found from the ſame equation, when λ, ω and D are given. For if the baſe of a ſpherical triangle be $= d$, the two other ſides $= 90° - \lambda$, $90° - \phi'$, and the contained angle $= \omega$, the coſ. ω will be $=$

$\frac{1 - \frac{D^2}{2a^2} - \text{ſin.}\ \lambda\ \text{ſin.}\ \phi,}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi'}$. Now let $\phi = \phi' + x$, where x muſt be very ſmall, and coſ. λ coſ. ϕ' there reſults coſ. $\omega' =$ coſ. ω + $\frac{(\text{coſ.}\ \lambda\ \text{ſin.}\ \phi'\ \text{coſ.}\ \omega - \text{ſin.}\ \lambda\ \text{coſ.}\ \phi')}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi'} \times \text{ſin.}\ x$: conſequently

$$\frac{(\text{coſ.}\ \lambda\ \text{ſin.}\ \phi'\ \text{coſ.}\ \omega - \text{ſin.}\ \lambda\ \text{coſ.}\ \phi')}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi'} \times \text{ſin.}\ x = \frac{c}{a} \left(2\,(\text{ſin.}\ \lambda - \text{ſin.}\ \phi')^2 - \frac{D^2}{2a^2}\ (\text{ſin.}\ \lambda^2 + \text{ſin.}\ \phi'^2) \right) \text{ nearly,}$$

and $x = \frac{c}{a} \times$

$$\frac{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi' \left(2\,(\text{ſin.}\ \lambda - \text{ſin.}\ \phi')^2 - \frac{D^2}{2a}\,(\text{ſin.}\ \lambda^2 + \text{ſin.}\ \phi'^2) \right)}{\text{coſ.}\ \lambda\ \text{ſin.}\ \phi'\ \text{coſ.}\ \alpha - \text{ſin.}\ \lambda\ \text{coſ.}\ \phi'}.$$

From the inveſtigation of Mr. Playfair's problem, therefore, we have obtained very accurate rules for finding ω from λ, ϕ and D, and ϕ from λ, ω, D.

Now in order to find an equation expreſſing the relation between the latitudes, difference of longitude, and one of the azimuths, let AL be perpendicular to the meridian PAO in A meeting FE in L, and BK perpendicular to AL. Join KE, and the angle BKE will be equal to the ſpheroidal angle OAB, and BFE equal to the angle APB or difference of longitude. Let OAB = BKE = A, BFE = ω, and λ, ϕ as before, then will LE be = (CD — CF) cotang. λ + FE — AD, KE = (CD — CF) coſ. λ + (FE — AD) ſin. λ, and cotang. A $= \frac{KE}{BE}$.

Whence by ſubſtituting the valves of CD, CF, AD, BF given above, and rejecting the powers of c higher than the firſt, there reſults

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reſults cot. $A = \frac{\text{coſ.}\,\phi\ \text{ſin.}\,\lambda\ \text{coſ.}\,\omega - \text{ſin.}\,\phi\ \text{coſ.}\,\lambda}{\text{coſ.}\,\phi\ \text{ſin.}\,\omega} - \frac{2c}{a} + \frac{(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)\ \text{coſ.}\,\lambda}{\text{coſ.}\,\phi\ \text{ſin.}\,\omega}$. But if ω be the vertical angle of a ſpherical triangle, and $90^\circ - \phi$, $90^\circ - \lambda$ the ſides; alſo A' the ſupplement of the angle oppoſite the ſide $90^\circ - \phi$; then will cot. $A' = \frac{\text{coſ.}\,\phi\ \text{ſin.}\,\lambda\ \text{coſ.}\,\omega - \text{ſin.}\,\phi\ \text{coſ.}\,\lambda}{\text{coſ.}\,\phi\ \text{ſin.}\,\omega}$, and conſequently cot. $A =$ cot. $A' - \frac{2c}{a} + \frac{\text{coſ.}\,\lambda\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)}{\text{coſ.}\,\phi\ \text{coſ.}\,\omega}$, Whence as A, A' are nearly equal, we obtain $A = A' + \frac{c}{a}\cdot 2\,\text{ſin.}\,A'^2 \times \frac{\text{coſ.}\,\lambda\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)}{\text{coſ.}\,\phi\ \text{coſ.}\,\omega}$, which will be found abundantly accurate in practice, but if the ſquare of c be retained, and $\frac{2\,\text{ſin.}\,A'^2\ \text{coſ.}\,\lambda\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)}{\text{coſ.}\,\phi\ \text{ſin.}\,\omega}$ be put $= M$, and $\text{ſin.}\,A'^2\ \text{coſ.}\,\lambda \times \frac{\text{ſin.}\,\lambda\ \text{coſ.}\,2\lambda + 2\,\text{ſin.}\,\lambda\ \text{ſin.}\,\phi^2 - \text{ſin.}\,\phi}{\text{coſ.}\,\phi\ \text{ſin.}\,\omega} - \text{cot.}\,A \times M^2 = N$, A will be $= A' + M \times \frac{c}{a} - N \times \frac{c^2}{a^2}$ more accurately. The rule may be thus expreſſed; let the colatitudes of the two places, and their difference of longitude form the ſides, and contained angle of a ſpherical triangle, of which find the baſe angle at the place whoſe latitude is λ, and let it be $= \alpha'$, and the correſponding angle α of the ſpheroidal triangle will be $= \alpha' - \frac{c}{a} \times 2\,\text{ſin.}\,\alpha'^2 \times \frac{\text{coſ.}\,\lambda\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)}{\text{coſ.}\,\phi\ \text{coſ.}\,\omega}$. In like manner if β' be the angle of the ſpherical triangle at the place whoſe latitude is ϕ, the correſponding angle β of the ſpheroidal triangle will be found to be $= \beta' - \frac{c}{a} \times 2\,\text{ſin.}\,\beta'^2 \times \frac{\text{coſ.}\,\phi\,(\text{ſin.}\,\phi - \text{ſin.}\,\lambda)}{\text{coſ.}\,\lambda\ \text{coſ.}\,\omega}$. Conſequently $\alpha + \beta$ is $= \alpha' + \beta' - \frac{c}{a} \times \frac{\text{ſin.}\,\lambda - \text{ſin.}\,\phi}{\text{coſ.}\,\phi\ \text{coſ.}\,\omega} \times 2\,\text{ſin.}\,\alpha'^2\ \text{coſ.}\,\lambda - \frac{c}{a} \times 2\,\text{ſin.}\,\beta'^2 \times \frac{\text{coſ.}\,\phi\,(\text{ſin.}\,\phi - \text{ſin.}\,\lambda)}{\text{coſ.}\,\lambda\ \text{coſ.}\,\omega} =$ $\alpha' + \beta' - \frac{c}{a} \times \frac{2\,(\text{ſin.}\,\alpha'^2\ \text{coſ.}\,\lambda^2 - \text{ſin.}\,\beta'^2\ \text{coſ.}\,\phi^2)\,(\text{ſin.}\,\lambda - \text{ſin.}\,\phi)}{\text{coſ.}\,\lambda\ \text{coſ.}\,\phi\ \text{coſ.}\,\omega}$; but by

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ſpherics ſin. ω' coſ. $\lambda =$ ſin. β' coſ. ϕ, therefore $\alpha + \beta = \alpha' + \beta'$ if we reject the powers of c higher than the firſt, which are inſenſible. Hence the principle laid down by Mr. Dalby, viz. that in a ſpheroidal triangle, of which the angle at the pole and the two ſides are given, the ſum of the angles at the baſe is the ſame as in a ſpherical triangle, having the ſame ſides, and the ſame vertical angle, is verified, and therefore the concluding remark of Mr. Playfair is haſty and ungrounded. But perhaps Mr. P. in his ſolution retains the ſecond power of c, and objects to Mr. Dalby's principle becauſe its coefficient does not vaniſh except in particular caſes. If ſo, the objection is frivolous, as the difference is ſo ſmall as ſcarcely to be computed in the caſes that occur in practice, and too ſmall in any caſe to lead into error or deſerve attention.

The preceding theorems for the ſolution of ſpheroidal triangles will be found extremely accurate, when applied to ſuch as are deſcribed on the ſurface of the earth, on account of the ſmallneſs of c in compariſon of a; and in like manner others may be deduced, when different parts of the triangle are ſuppoſed given. Thus if λ, α and D be given; let a ſpherical triangle be conſtructed with one ſide $= 90° - \lambda$, another $= d$, ſuch that ſin. $\frac{1}{2} d = \frac{D}{2a}$, and the contained angle $= \alpha$; find the other ſide $90 - \phi'$, the angle at the pole ω', the other azimuth β' and we ſhall have equations of this form $\phi = \phi' + Pc$, $\omega = \omega' + Qc$, and $\beta = \beta' + Rc$, where P, Q, R are functions of λ, α, D, which may be derived from the foregoing equations by proper artifices. But the formulæ, except in particular caſes, will not be found ſo ſimple as the former. Theſe, however, and ſome new theorems applicable to trigonometrical ſurveys, I ſhall delay to ſome future communication. In the mean time, it may not be foreign to the ſubject to remark, that the arch of the meridian, ſaid to have been lately meaſured in the Myſore country in the Eaſt Indies, by Brigadier Major Lambton, gives the degree, in latitude 12°.32′N. equal to 60194 fathoms; which compared with that of 60795 in latitude 47°.24′ N. gives $\frac{1}{300}$ for the compreſſion at the poles, a quantity differing very little from the mean deduced from all the meaſures of degrees. But it muſt be confeſſed that there appear at preſent to be two very important objections againſt the accuracy of Major Lambton's meaſure. The Myſore, on

Arch of the meridian meaſured in the Myſore country, account of the irregularity of its ſurface and its uncertain elevation above the level of the ſea, is an unfit country for aſcertaining a nice point of this kind, however well ſituated for connecting the eaſtern and weſtern ſides of the peninſula by a geographical ſurvey; and the Major, from his account in the 7th volume of the Aſiatic Reſearches, ſeems to be ſomewhat doubtful of the exact length of his chain. Nevertheleſs it is probably to India that we muſt look for the means of finally deciding this long conteſted queſtion. There, and there only, we find many tracts of country highly favourable to this purpoſe; and it is to be hoped that the Eaſt India Company, while anxious to aſcertain the extent of its poſſeſſions, will not entirely neglect the intereſts of ſcience.

—perhaps in ſome degree uncertain from cauſes ſtated.

I am, Sir, &c.

PERIGRINUS PROTEUS.

Portſmouth, April 7, 1804.

IV.

Deſcription and Drawing of a Hand Drill for ſowing Peas, Beans, &c. Communicated to the Society of Arts, by the Inventor, Mr. ROBERT GREEN, *of Weſtwratting, Cambridgeſhire*[*].

To CHARLES TAYLOR, Eſq.

SIR,

Very economical drill for ſowing peas.

I HAVE invented an engine to ſow peas, with which I have ſown all my peas, to the amount of 40 acres, at the price of 1*s.* per acre, and think that my peas are much better than thoſe ſown any other way. It is alſo on a very ſimple plan, and the expence of it when complete is not 2*l.* It is uſed by manual labour, without any horſe; and it will draw the drill, ſow the peas, and cover them at the ſame time, and will ſow them much rounder than any other I have yet ſeen. I likewiſe find I can do it much cheaper than with any horſe, and am of opi-

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[*] To whom the ſilver medal and ten guineas were voted by the Society. A complete machine is placed in the repoſitory of the Society.

nion

Very economical drill for ſowing peas.

nion that it ſows much better than any drill I have ſeen. If the Society wiſh it, I will ſend a model for their inſpection.

I am, Sir,

Your obedient Servant,

ROBERT GREEN.

Weſtwratting, Cambridgeſhire,
June 27, 1802.

SIR,

I have ſent the engine for ſowing peas, in order that it may be laid before the Society for the Encouragement of Arts, &c. I intended to have ſent a model of it, but afterwards thought that the engine itſelf would be more acceptable to the Society. I made it myſelf, and have ſown with it 26 acres of land in my own occupation. Mr. Piper, a near neighbour of mine, has ſown with it five acres; and Mr. Cock, of Blunt's Hall, Wratting, in Suffolk, 25 acres, at the expence of 1*s*. per acre. Several other gentlemen had drills of me for ſowing peas. If I give my men 1*s* 6*d*. per acre, they will ſow for me two acres in one day. I can with my own hand ſow one acre in five hours, and at the ſame time ſow the peas, draw the drill, and cover them, and make full twelve drills and a half to the rod. I likewiſe produce the plant much handſomer than any other ſeen in our country, and at a very trifling expence. By this too, the labour of horſes is ſpared, which we find to be a very material circumſtance. It will be a moſt excellent engine for gardeners in the neighbourhood of London; for I will be bold to ſay, that no man can ſow with his hand, ſo as to equal this, at a very trifling expence.

I have ſpent much time in making implements of huſbandry, but have made none ſo uſeful as this; for it is ſimple in its conſtruction, may be purchaſed by any man, the expence being ſo trifling, and ſaves the labour of horſes.

I remain, Sir,

Your moſt obedient Servant,

Weſtwratting. ROBERT GREEN.

Deſcription

Deſcription of the Engraving of Mr. ROBERT GREEN'S *Hand-Drill, for ſowing Peas, Beans, &c.* Plate II.

Very economical drill for ſowing peas.

Fig. 3. *a a,* The wheels placed upon a wooden axis *b*, which is ſquare at each end, but round in the centre. The ſquare ends of the axle have holes throughout them, at different diſtances in order to depoſit the ſeed at nearer or more diſtant intervals, as may be wanted.

c, The box in which the ſeed is placed: the axis *b* is cylindrical, and has holes made therein proper to receive the ſeeds, which by the revolution of the axis are carried forwards, and fall through an iron tube into the ground opened for them by the ſhare *d*. When depoſited in the ground, they are covered, or the earth drawn over them by two iron pins or ſcrapers *e*, fixed on each ſide of the tube, and extending ſome inches behind it.

f f, The handles of the drill-machine, by which it is puſhed forwards.

Fig. 4. Shows an enlarged view of the interior of the ſeed-box *c*, above mentioned, and holes for the ſeeds placed in a ſpiral line, in order to drop the ſeeds more regularly.

g, Is a ſmall bruſh within the box, which rubs againſt the cylinder, to keep the holes clear to receive the ſeeds.

Fig. 5, Is a ſection of the machine, where *a* is part of the ſeed-box; *b*, the round part of the axle, which delivers the ſeed.

d, The ſhare which opens the earth.

h, The tube through which the ſeed falls.

i, The mouth of the tube, and one of the fins which draws together the ſoil, and covers the ſeed.

k, Is a ſmall door, to be opened occaſionally, if the roller or tube are out of order.

l, A ſtrong flat board, to which the iron work is ſcrewed.

Fig. 6, Shows an enlarged plan of the iron work, when the machine is reverſed.

d, Is the ſhare.

i, The hole from which the ſeed is dropped.

e e, The two fins, or ſcrapers, which collect the earth and cover the ſeed.

N. B. The length of the upper rim of the feed-box of the machine in *Fig.* 3, being fifteen inches, will ſerve as a ſtandard for the meaſure of the other parts *.

V.

Enquiries concerning the Methods of inveſtigating the Courſe and Velocities of a Body ſuppoſed to be projected from the Moon to the Earth. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

Theory of La Place, that ſtones fall from the moon.

AMONG the various theories concerning the ſtones fallen upon the earth in different parts of our globe, (the ſubjects of Mr. Howard's Analyſis), that which De la Place has ventured upon, though apparently incapable of proof, ſeems the leaſt improbable, namely, that they are projections from the lunar volcanos. Surely, Sir, whether this is or is not *poſſible,* may be mathematically demonſtrated, certain data being allowed. If you, or any other able mathematician, could find time to do this, it would be a great gratification to ſee the ſolution of theſe queſtions.

Data for computation.

We muſt aſſume as data, That the denſity of the materials compoſing our ſatellite, is preciſely ſimilar to the denſity of our globe; and that the moon has no atmoſphere to reſiſt the projection from its ſurface, or ſo ſmall a one as not to be calculated upon, ſince, probably, it is rare and low, perhaps not more than one-ſixth of a mile high.

Now their relative bulks and diſtances are *pretty well* aſcertained, and we will take them to be *preciſely* known.

Their comparative centrifugal forces may eaſily be calculated, as the moon revolves about 27 times ſlower about its axis than the earth about hers.

Theſe points muſt be ſettled, becauſe particularly the laſt muſt have conſiderable effect in determining the line ſuch projected body would deſcribe.

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* This account was alſo ſupported by a certificate from eight perſons who had uſed the drill in ſowing 113 acres of land, and of eleven farmers who witneſſed and approved its operation.

Let us ſuppoſe too, that this body weighs one hundred weight.

Reſults to be inveſtigated.

What will be the velocity requiſite to overcome the mutual attractions of the moon and this body, ſo as to project it beyond their powers; remembering that its velocity will be continually diminiſhing as long as any attracting power acts upon the projectile, and calculating the aid it would receive from the centrifugal force.

In the journey of 240,000 miles through which it has to travel in a direct line, is there any free ſpace beyond the ſphere of the moon's attraction and that of our planet's? If ſo, at what diſtance from the moon will that be found? and at what diſtance from the earth? and with what velocity may it be ſuppoſed to travel through that ſpace? of courſe it will be much ſlower than when firſt projected.

As upon entering the limits of the earth's attraction its velocity will be again increaſed, *quære* its rate of travelling to the earth; and, taking the three reckonings into account, in how many days and hours can it be mathematically demonſtrated that it would reach our ſolid globe?

Acted upon by the united forces of projection, centrifuge, (if I may coin a word) and the motion of the moon in its orbit, and the force of attraction and orbital motion of the earth, what will be the preciſe line it may be preſumed to deſcribe in its courſe?

Would it not have a revolving motion during ſome part of its courſe?

What are the calculations by which we may be enabled to judge that 3 or 5 times the velocity of a cannon-ball, at the moment of projection, would enable it to counteract theſe impediments?

It has ſurprized me, that the numerous late publications mentioning this theory, have not detailed the mathematic proceſſes by which it ſeems capable of being ſolved, or of proving its fallacy.

I hope, Sir, you will not think this obtruſion impertinent. You obligingly and ſatisfactorily complied with my requeſt in a note concerning Col. Blaquiere's gun to throw double headed ſhot; this has emboldened me to expreſs my wiſhes on the ſubject of this letter. But I do not wiſh to have my ignorant queſtions load your valuable Journal, though I ſhall feel myſelf much obliged by any further information upon this head.

I remain, Sir,
Your moſt humble ſervant,
A CONSTANT READER.

VI.

Drawing and Deſcription of a Three-Furrow Plough. By the Rev. EDMUND CARTWRIGHT, *of Woburn, Bedfordſhire.*[*]

To CHARLES TAYLOR, Eſq.

DEAR SIR,

Economical three-furrow plough.

I ENCLOSE you a certificate of the performance of a plough of my invention, which has occaſionally been at work through the whole ſummer. For this laſt fortnight, it has been uſed for ploughing in wheat under furrow. Though a very uſeful inſtrument at all times, it is particularly valuable at the ſeed times, and the turnip ſeaſon; becauſe at thoſe times it frequently happens you loſe the moſt favourable opportunities, for want of ability to execute your operations with ſufficient diſpatch.

Weekly ſaving 4l. 16s.

I need not calculate to you the ſaving on the uſe of this plough. It is worked (on light land I mean) with a pair of horſes, without a driver. A pair of horſes and a ploughman cannot be laid at leſs than 8s. per day. As two ſets of theſe are ſaved, the weekly ſaving by the uſe of this plough amounts to no leſs than 4*l.* 16*s.*

Applicable to level lands in a tolerable ſtate of cultivation.

Uſeful, however, as I find this inſtrument on our light level lands, I am not ſo partial to it, to ſuppoſe it is equally calculated for all ſoils, or all kinds of ground. For inſtance, where the ground is very uneven, or the ridges are narrow and ſteep, I would not uſe it; neither when the land is very foul with root weeds. In all theſe caſes a ſingle plough is certainly to be preferred: but in all caſes where the ground is in a tolerable ſtate of cultivation, and where it lies reaſonably level, it will be found a moſt valuable acquiſition.

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[*] From the Tranſactions of the Society of Arts, who voted him the ſilver medal. A model is placed in their repoſitory.

I will thank you to communicate this letter, and the certificate accompanying it, to the Committee of Agriculture; and and if they are diſpoſed to think favourably of this invention, I will ſend you a model for their inſpection.

I am, DEAR SIR,
Your very obedient ſervant,
EDMUND CARTWRIGHT.

Woburn, Oct. 20, 1802.

Certificate.

This is to certify, that the three-furrow plough invented by the Rev. Edmund Cartwright, ploughs a ſurface of twenty-ſeven inches each bout, and that on light land a pair of horſes regularly ploughs three acres per day with it in a workmanlike manner.

JOHN DUCKITT, *as Bailiff to his Grace the Duke of Bedford.*
WILLIAM BAXTER, *Aſſiſtant.*

June 21*ſt*, 1802.

DEAR SIR,

Why this plough ſaves power.

YOU herewith receive the model of my three-furrow plough.

The ſaving of hands, and conſequently of expence, in a plough of this kind, is obvious; but why there ſhould be a ſaving of power, may require to be explained.

I need not obſerve to you, nor to any man who conſiders the action of a common plough, that a very material part of the labour in ploughing, ariſes from the friction of *the land ſide* and *the ſole;* of the one againſt the ſide of the furrow, of the other againſt the bottom. In a ſingle plough a certain length and width are required in thoſe parts of it, to make it go ſteady; and even then the effect would be imperfectly obtained, did not the ploughman aſſiſt by the leverage of the handles of the plough. Hence it is clear, that the leſs diſpoſition any plough has to follow the draught in a ſtrait line, the greater is the labour of working it, becauſe the ploughman in that caſe is to exert a greater power of leverage to keep it ſteady. On the contrary, when two, three, or more ploughs are combined, they ſerve to ſteady each other, and require comparatively very little power of the lever to keep them in a ſtrait line. Under theſe circumſtances, Economical three-furrow plough.

circumſtances, neither the firſt nor ſecond plough has any ſole or land-ſide whatever; and even the third does not require ſo much of either as a ſingle plough. I calculate the ſaving of power from the conſideration alone, as equal at leaſt to one plough. What farther power is ſaved, I attribute to the lightneſs and compactneſs of the inſtrument.

I am willing to think the ſimplicity of its conſtruction, and the manner of fixing the plough (conſiſting but of two parts) to the beam, will not eſcape your obſervation. When the cutter (for as it is both coulter and ſhare, I can give to it no other ſingle name) requires to be ſharpened, or new-laid with ſteel, by drawing the two bolts the whole is ſet at liberty.

I make the ploughs to fit each beam indiſcriminately; becauſe when the land is too ſtrong, or too foul, to work the three, I take off the ſecond plough, and transfer the third into its place.

You will obſerve the centre of the whiple-tree ſhifts. By this contrivance, the power of the horſes is equalized, though they may be unequal in ſtrength, the longer lever being given to the weaker horſe.

Should the Society wiſh for any farther information, it will give me pleaſure to furniſh them with it.

I am, Dear Sir,
Very truly and ſincerely, yours,
EDMUND CATWRIGHT.

Woburn, December 14, 1802.
Charles Taylor, Eſq.

Reference to the Engraving of the Rev. Edmund Cartwright's *Three-Furrow Plough.*—Plate II. Fig. 1, 2.

Deſcription of Mr. Cartwright's three-furrow plough.

Fig. 1. A B, the two wheels of the plough, the wheel B being full one-ſeventh in diameter larger than the wheel A.

C D E, the three beams of the plough, of which C is the ſhorteſt and E the longeſt: theſe beams are fixed in the ſtrong croſs piece F, at equal diſtances from each other, and braced by another croſs piece from C to E.

G H I, the three cutters which anſwer the purpoſe of both coulter and mould-board, each being formed together, or made of one piece of beaten iron. Each cutter is ſcrewed to its beam by the flanging-iron K.

L M

Deſcription of Mr. Cartwright's three-furrow plough.

L M, the two handles of the plough, the lower extremities of which are fixed in the two outer beams C E, and connected by a croſs piece N, to make them firmer. The handle L is longer than the handle M, in the ſame proportion as the beam C is ſhorter than the beam E.

O P, two upright pieces of iron fixed in the croſs piece F, having two holes at their ſummits for the reins to paſs through which guide the horſes.

S, an iron bar which ſlides up and down near one end of the croſs piece F, to raiſe or lower the wheel A.

Fig. 2. Shows a detached portion of the ſtrong croſs piece F, to explain the manner in which the whiple-tree ſhifts (R) are fixed in front of that croſs piece, ſo as to regulate or equalize the power of the horſes.

S, a bar of iron, the lower part of which forms the axis of the wheel A, the upper part ſlides in a groove, in the croſs piece, F, and has holes at different diſtances. It may be retained at any height by an iron pin T, which paſſes through the croſs piece, and one of the holes of the iron bar. The real plough is nine feet long to the extremity of the handles and each cutter turns a nine-inch furrow; from centre to centre of the beams, being nine inches.

VII.

On the State of Science among the earlier Nations of Antiquity: and more eſpecially of thoſe Reſearches which conſtitute the Subjects of Alchemy. In a Letter from E. P.

To Mr. NICHOLSON.

Roſcommon, April 1, 1804.

SIR,

Reaſons for thinking that the modern ſciences were known to antiquity.

IT is an authenticated fact, that much of our late ſcientific acquiſition, and many of thoſe facts which the experimental genius of the preſent age is daily bringing to light as original diſcoveries, were well known in more ancient periods of the world; and there is abundant reaſon for ſuppoſing that, in chemiſtry and metallurgy, the philoſophers of thoſe ages were ſuperior to thoſe of the preſent day.

But we muſt mount up much higher than what are called the dark and barbarous ages of modern Europe, or even of any of thoſe revolutions in the Eaſt of which hiſtory has tranſmitted any detailed accounts.—Science had *began to decline previous* to the earlieſt hiſtoric relation which is extant, and there appears ſufficient evidence that the Greeks and Egyptians, in their hieroglyphics, their allegoric devices, and in their mythologic myſteries, which they had blindly received from their enlightened predeceſſors, were recording for poſterity a ſeries of phyſics, of which they were ignorant, and which is now gradually unfolding.

Tranſmutation of metals.

The univerſal rage for penetrating into the ſcience of alchemy, not only indicates the ſcarcity and value of gold in all ages, but evinces, I think, that there has always exiſted ſome *tradition* of ſuch a tranſmutation having been once effected.

Decompoſition of water.

The decompoſition of water into different gaſes was certainly once known; and our recovery of that ſublime phenomenon, which ſeems the key to the great laboratory of nature, bids fair to reſtore to mankind the moſt important facts which have lain in obſcurity for ſo many centuries.

Alchemy.

Of theſe, alchemy will probably be one; it has deeply excited the attention of ſome excellent chemiſts in this iſland, with whom I have the honour of being connected: of any progreſs we may hereafter make, you ſhall be immediately appriſed; and if you, or any of your ingenious correſpondents are engaged in a ſimilar courſe of experiments, we might mutually aſſiſt, and abridge each others labours.

I have the honour to be,
Your ſincere friend and zealous well-wiſher,
E. P.

P. S. A ſeries of experiments on this ſubject will probably throw conſiderable light on the lunar (more properly lunatic) ſtones, the rational phenomena of which you have taken ſo much laudable pains to elucidate.

VIII.

Deſcription of a Machine for Clearing great Roads from Mud. Communicated to the Society of Arts, by the Inventor Dr. JOHN WINTERBOTTOM, *of Newbury, Berks**.

Machine for clearing roads from mud.

IN a deſcription of this machine, I ſhall briefly notice the five principal parts of which it is compoſed; the frame, the ſcraper, the chain, the ſledge, and the pole; becauſe a very accurate model accompanies this paper, made upon the uſual ſcale of one inch to a foot.

The *frame* (ſee *Plate* I. *Fig.* 1.) conſiſts of two pieces of timber AA, which at one extremity are formed into a pair of ſhafts BB, and at the other are ſtrongly united by three tranſverſe pieces CDE.

The *ſcraper* F is placed under this frame-work, in an oblique direction, at an angle of 30°, between two of the tranſverſe pieces CD, and conſequently forms an angle of 150° with the line of draught. By this poſition of the ſcraper, the machine, when uſed, actually clears itſelf from the mud as faſt as it is collected, and removes it into a heap on one ſide, after the manner of a plough.

The *chain* G is connected with a piece of iron-work H, which projects from the lower end of the ſcraper; for here additional power is required, as the whole body of the mud, which has been collected, muſt paſs off by this extremity. Some advantage has alſo been gained by making this end of the ſcraper ſhorter than the other.

The *ſledge* II is conſtructed upon the upper part of the frame, that by inverting the machine it can be tranſported without injury to the ſcraper, over the moſt rough and ſtony roads, or pavements, to thoſe places where its uſe is particularly required.

The *pole* K, which is moveable, ſerves the purpoſe of a rudder, that when the machine happens to be forced by any great weight of mud, or ſolid body of earth, &c. from its proper direction, it can be eaſily reſtored to its former poſition: and it may alſo be obſerved, that the moderate preſſure

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* To whom the ſilver medal was voted. There is a model in the Society's Repoſitory.

Machine for clearing roads from mud.

of the hand upon the pole tends to make the machine ſteady, and therefore cauſes it to work to more advantage. In the model, the pole is made only ten inches long, inſtead of fifteen, that it might occupy leſs ſpace in the box. The plates in front of the ſcraper, and upon the ſledge, are made of caſt-iron.

OPERATION:

For the operation of the machine, two men and four horſes are required: one man to drive the horſes, and another to take the management of the pole and the direction of the labour to be performed. The horſes are to be worked double, as commonly practiſed, two being employed to draw by the ſhafts, and two by the chain above deſcribed. But the manner of uſing the machine will be beſt underſtood by the following ſketch. *Plate* I. *Fig.* 3.

The firſt progreſs of the machine marked No. 1, commencing from the arrow-mark, will remove the mud in a line to the right; the firſt return, No. 2, will remove another part of it to the left. The ſecond progreſs, No. 3, will take up what is left by No. 1, beſides the quantity which is upon the ſpace now to be paſſed over, and will remove it all to the right. The ſecond return, No. 4, will operate in a ſimilar manner with regard to No. 2, and remove that to the left. Thus, by four lengths, more than twenty feet wide of a road can be cleared; and this has been frequently performed in the preſence of ſeveral perſons. The number of lengths may be increaſed at pleaſure, according to the width of the road.

In the neighbourhood of London, where there is inceſſant travelling, it would be adviſeable to uſe two machines at the ſame time, one immediately following the other, as in No. 1 and 3, which will leave a ſpace ſufficiently wide for the largeſt carriage to paſs, without diſturbing the mud already ſcraped up.

There is one advantage in the operation of this machine worthy of being noticed, which is, that by the uſe of it the road is made more even and ſmooth, the ſmall holes being filled up by the more ſolid parts of the mud; whereas, when roads are ſcraped in the uſual way, by hand, all the irregularities are increaſed, and become the future depoſits of water; and it is univerſally known that theſe puddles, as they are called, are the chief cauſe of the deſtruction of roads.

Machine for clearing roads from mud.

'It has been obſerved, that ſtones are ſometimes forced up by the machine; but it appears to be thoſe only which project in ſuch a degree as to be dangerous to the traveller, and which require to be broken for the more effectual mending of the road.

I can ſay nothing concerning the effect of the machine upon duſty roads, having had no opportunity of trying it at that ſeaſon of the year. When, indeed, the roads are watered, as about London, there is no doubt but a great quantity of that dirt may be removed, which, in a few hours of ſcorching ſun, would again be converted into a body of duſt.

If it ſhould be objected, that the machine is too large, and that a ſmaller one, which might paſs over half the ſpace of ground that this does, and might be worked by two horſes, would be better; I muſt beg leave to anſwer, that, in my opinion, with a leſs one there would be much labour to little purpoſe; becauſe this machine, which paſſes over a ſpace of about ſix feet and a half, will not, in ſome places, when the roads are very wet and very deep, leave more than three feet clear, the mud on each ſide falling in and filling up, to a conſiderable extent, the ſpace already paſſed over: it muſt therefore be obvious, that, under ſimilar circumſtances, the track of a ſmaller one would almoſt inſtantly be obliterated.

TESTIMONIALS.

I am ſo anxious that the Society ſhould have ample ſatisfaction on this head, that I ſhould be happy if they would, before finally determining on the utility of this machine, condeſcend to make ſome inquiries in this part of the country, where it has been publicly tried.

I can however mention, with ſome pleaſure, that ſeveral gentlemen, acting as Commiſſioners of the Roads, have honoured me with their attendance during various experiments; and, having witneſſed the very powerful effects of the machine, they have given it their public approbation at the laſt monthly meeting, when the following entry was made in their minute-book:—

" At a meeting of Truſtees of the London and Bath roads, held at the Globe Inn, Newbury, on Monday, the 21ſt of February, 1803. At this meeting were preſent, James Croft, Eſq.

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Eſq. Frederick Cowſlad, Eſq. Rev. Thomas Beſt, Mr. Richard Baily, Mr. Thomas Clark, Mr. John Baily, Mr. Joſeph Tanner, Mr. Thomas Pocock.

"Reſolved, that the machine invented by Dr. Winterbottom, for ſcraping off mud from turnpike roads, will be of public utility, and ſave conſiderable expence of labour."

After this public teſtimony in its favour, I might perhaps be excuſed from producing the certificates of a few individuals: it will, notwithſtanding, be neceſſary to give ſome eſtimate of the probable ſaving to be expected from its uſe.

In all trials made previous to the 21ſt of February, the machine had been worked upon no meaſured extent of ground; but the general effects were ſuch, that ſeveral perſons of great experience in the management of roads, rated the daily work of one machine only as equal to the labour of fifty or ſeventy men: fifty being the loweſt eſtimate ever named.

A few days ago I directed ſome work to be done by meaſure; and I can now ſtate it as the opinion of two very competent judges, that one machine will clear three miles in a day, twenty feet wide (conſiſting of four lengths, and making the day's work twelve miles) which is conſiderably more than 120 men can do in a day.

	l.	s.	d.
120 men, at 2*s*. per day	12	0	0
Four horſes and two men can here be hired to work the machine for the day, at -	1	5	0
Difference - - -	10	15	0

At a diſtance, where carriages run principally in the centre of the road, the chief buſineſs in the management of it conſiſts in keeping the ſides clear and open. One machine may therefore be occaſionally employed in outſide work only; that is, may go ſix miles, and return, (making twelve miles, as juſt mentioned) with the ſaving already given.

Whatever ſurpriſe theſe calculations may occaſion, the Society will perhaps be ſatisfied that I have not over-rated them, when I produce the reſult of a fair experiment, made on the 25th of February, in the preſence of four truſtees (Frederick Page, Eſq. Francis Page, Eſq. Mr. Thomas

Clark, and Mr. John Baily) and others, by which it appears, that two miles, by meaſure, on the road to Reading, were cleared from mud, to the extent of 18 or 20 feet wide, by two machines, in the ſpace of two hours and a half, by the watch; and the work was judged to be equal to the labour of more than eighty men in a day.

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The ſucceſs of this experiment was ſo ſatisfactory to the above-named truſtees, for I was not preſent on the occaſion, that they directed, without my knowledge, the remainder of our diſtrict in this road, extending ſeven miles, to be cleared in the ſame manner; and I can now declare, with ſome degree of pleaſure, that this was actually completed by two machines in one day, viz. on the following day, the 26th of February. Of this day's work I have heard it affirmed, by an experienced ſurveyor, that it could not have been done in one day by 400 men.

I confeſs that I am myſelf unable, from the want of practical knowledge on this ſubject, to form a comparative eſtimate between the work done by this machine and by hand: I have therefore ſought for information from perſons of reſpectable characters, who have been ſurveyors, or renters of roads for many years: and I have been aſſured, as well by thoſe who were preſent at the experiments, as by others who examined the roads afterwards, that it would require ſixty men a mile, to do the work in one day, which a ſingle machine will accompliſh at four lengths; and it has been already ſhown, that three miles can, without difficulty, be cleared in a day: one machine will therefore do the work of one hundred and eighty men. But I have taken the average at only two thirds of this eſtimate, viz. at forty men per mile inſtead of ſixty, being more willing that the power of the machine ſhould at preſent be under-rated, than that the public ſhould be deceived or diſappointed concerning it.

The truſtees of the London and Bath roads, being deſirous of having theſe two machines, which had been conſtructed on my account, and under my own inſpection, for making the experiments, I have conſented to diſpoſe of them: and as far as I am now able to judge, the price of a machine complete will be about ten guineas.

Finally, I muſt beg leave to adviſe thoſe who are inclined to make a trial of this machine, to be careful whom they in-

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tend to employ in the conſtruction of it; for I can aſſure them, that it is not ſufficient to attend only to the form of the model; but it is abſolutely neceſſary that the different parts, and eſpecially the two braces behind, ſhould be firmly put together, otherwiſe it will be impoſſible for it to withſtand the force that muſt ſometimes be exerted upon it by four, or perhaps by ſix horſes. The ſcraper may be made of beech or elm, &c. but the other parts ought to be made of aſh; and I muſt particularly recommend theſe materials to be well ſeaſoned; all which circumſtances were minutely attended to in the two machines which were made for me by Mr. Joſeph Moſs, of Greenham, near Newbury.

JOHN WINTERBOTTOM.

Certificates from Mr. George Goddard, Greenham, near Newbury, Mr. Francis Page, Mr. Frederick Page, Mr. John Baily, and Mr. Thomas Clark, accompanied the above paper; ſtating, that on the 25th of February laſt, two miles had been cleared in two hours and a half, by two of Dr. Winterbottom's machines.

Reference to the Engraving of Dr. WINTERBOTTOM'S *Machine for Clearing Roads from Mud.—Plate* I.

Fig. 1, AA. Two pieces of aſh timber, forming at one extremity a pair of ſhafts, BB.

CDE. Three tranſverſe braces, to ſecure firmly the timbers above-mentioned.

F. The iron plate, or front of the ſcraper, fixed within the braces C D, at an angle of thirty degrees, extending on the further ſide two feet, and on the nearer ſide one foot and a half beyond the timbers.

G. An iron chain, one end of which is faſtened to the outſide of the timber A; the other end of the chain may be moved nearer to, or further from that end of the ſcraper which depoſits the mud, by means of notches in the iron muzzle H, fixed to the ſcraper, and which regulates the draught of the horſes attached to the ring at G.

K. The pole, or handle, to be made fifteen feet long, which paſſes through the ſtrong holdfaſts in the braces C D. This pole acts as a lever, as the ſcraper may be raiſed or ſunk by

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by it, at pleaſure. The perſon who holds it may direct the ſcraper in its proper line, and aſſiſt it in overcoming any obſtacles it may meet with in its way, or in giving it additional preſſure where neceſſary.

II. Show the two parts of the machine which form the feet, or ſledge part of the machine, on which it ſlides when reverſed, and which enable it to be removed from place to place, when the ſcraper is not in uſe. Theſe feet are ſtrongly fixed to the timbers AA, and ſtrengthened by a tranſverſe brace betwixt them.

L. Is the iron chain, or back band, which lies upon the cart-ſaddle of the horſe in the ſhafts, and which ſupports the ſhafts.

Fig. 2. Shows, on an enlarged ſcale, the iron-work, fixed on the outſide of the ſhafts, to which the chain and horſe are attached.

Fig. 3. Deſcribes, in a ſmall extent, the track uſually made by the ſcraper in a large way, in four rows, commencing at the arrow mark, in the track No. 1, returning after it has gone any length required by the track No. 2, proceeding again by the track No. 3, and forcing the mud collected by the tracks No. 1 and 3 to the right ſide of the road, and, on its return by the track No. 4, depoſiting the mud of the tracks No. 2 and 4 on the left of the road, as is more fully deſcribed in the preceding account, and thus clearing from mud a breadth of road twenty feet wide, by four paſſages of the machine.

IX.

Deſcription and Drawing of an Hydraulic Machine, with an Account of ſeveral Inventions of early Date, which have been ſince brought forward by later Inventors. Extracted from a Foreign Work publiſhed early in the laſt Century. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

Account of the publication of the Muſeum of Monſ. de Serviere.

I HAVE lately met with a work in French, entitled *Recueil d'Ouvrages curieux de Mathematique et Mecanique, ou Deſcription du Cabinet de Monſ. Grollier de Serviere,* in quarto, printed

Turnery.

Time-pieces.

Hydraulic machines.

printed at Lyons in 1719. The book is divided into three parts, the firſt of which contains curious engravings of delicate, eccentric, ſwaſh and roſe-work turnery, the methods of producing which are to be found in Moxon's Mechanic Exerciſes, and in other later works on that ſubject. Upon this part I ſhall make no other remark, than by demanding of your correſpondents, whether the oval chuck, or engine for turning ovals, was unknown ſo late as the period above ſpecified. If Monſ. de Serviere had known it, I ſhould ſuppoſe he would have introduced ellipſes among the various figures he has exhibited. The ſecond part of the work conſiſts of clocks or time-pieces, more remarkable for ſome ſingularity in figure or ſtructure, than any improvement in the art of meaſuring time; and, the third part contains models of hydraulic machines, with ſome engines for military and other purpoſes. Many of the hydraulic machines appear better calculated to be ſhewn in a model, than carried into effect on a larger ſcale, and, among the other engines, I ſee nothing which, at the preſent day, would much conduce to the entertainment of your readers. I have, therefore, ſent you a drawing of the hydraulic engine exhibited in his 49th plate, and after the deſcription, I will mention a few other objects which have been thought of more modern invention.

Hydraulic machine deſcribed. It conſiſts of two pinion working in an elliptical box.

In *Plate* IV. *Fig.* 2. A and B repreſent two ſolid pinions made in wood or metal, and occupying all the interior ſpace of the oval box or chamber C D; in which they turn freely and take into each other. The chamber C D is to be well and ſolidly made, having an opening below at D, as in the figure, and alſo at E, where the aperture correſponds with the bore of a pipe F, applied and fixed to the ſame. Every other part is well cloſed and ſecured. *Fig.* 3, repreſents the cover or cap.

This chamber is properly fixed under the water of the well, or ciſtern, out of which the ſupply is to be obtained, and in this ſituation, the elbow or handle G, *Fig.* 1. *Plate* IV. is fixed on the ſquare of the axis A. This handle is connected with another H, by the iron ſliding piece I, which moves upon the fixed pin K, and obliges its two extremities conſtantly to move alike. Whenever therefore the handle H is turned by the firſt mover M L, the other arm G muſt alſo revolve together with its pinion, and by conſequence the other pinion B.

When

Its action.

When the two pinions turn (ſo that their upper teeth may conſtantly approach each other, while the lower recede,) the water which lies between their teeth in the lower part D of the chamber, will be carried round till it arrives at the part C, where it will be compreſſed by the continual augmentation of water, which is brought thither between the teeth, (or rather by the perpetual diminution of the ſpace between the two uppermoſt teeth that touch the chamber). So that the fluid will enter the pipe F, and be forced to the intended place.

Remarks on the engine.

Thus far I have tranſlated from Monſ. de Serviere. The machine appears to be ingenious, and though liable to the objections of wear which you very properly urged againſt the ſchemes of O. B. and others*, ſeems preferable to them in ſeveral reſpects. I think it would be an improvement, to cauſe the axis A and B to drive by a connection of external wheel-work, inſtead of depending upon their interior teeth, which require to be well figured and fitted, and kept ſo, by not loading them with preſſure of one ſurface againſt the other. It ſeems ſcarcely neceſſary to remark, that we have better methods of connection for work at a diſtance in our cotton gear and elſewhere, than the ſliding piece I; and laſtly I would propoſe it as a mathematical exerciſe to determine the law of the velocity of the fluid through F, when the rotation of the machinery is uniform.

Old engines re-invented.

Among the machines which I believe have been conſidered as of later date, but are found in this work, are the engine mentioned in Deſaguliers, for raiſing water by a loſing and gaining bucket, and regulated by a fly; the chain pump, for which Cole had a patent, but which is known to have been of very ancient uſe in the Chineſe Empire; the horſe mill worked by the wheels of a carriage, as lately propoſed by the ingenious Adam Walker; and the gouty chair of Merlin, worked by two ſmall handles connected with a pair of ſmall wheels attached to the fore feet.

Loſing and gaining bucket.

Chain pump.

Adam Walker's horſe mill.

I am, Sir, with eſteem,

Your obliged Reader,

R. B.

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* Philoſ. Journal. Quarto ſeries. IV. 468.

X.

An Examination of Dr. Wollaſton's Experiment on his Periſcopic Spectacles. *By Mr.* WILLIAM JONES, *F. Am. P. S.*

To Mr. NICHOLSON.

SIR,

Preliminary obſervations.

THE inferences that Dr. Wollaſton has thought it beſt to publiſh in your laſt month's Journal, inſtead of a direct reply to my refutation of his new principle of ſpectacle glaſſes, are of themſelves ſufficient to convince any impartial perſon of the validity of the objections advanced by me in your preceding Journal; and, notwithſtanding an extraordinary experiment he has therein related, as made only by himſelf, I ſhould not have thought it requiſite to trouble your readers again, but for the unfounded imputation that he has declared againſt me, that of having by an experiment deceived myſelf. I truſt, Sir, I may be allowed, in contradiction to this, to obſerve, that, after more than 20 years experience in the practice of my profeſſion, ſuch as daily adminiſtering to decayed viſion, and employment in the conſtruction of all kinds of optical inſtruments, I ſhould not be acquainted with all the various properties of lenſes, ſingly or combined, and eſpecially of ſo ſimple and well known a form of lenſes as adopted by him, is an idea, that I am confident he will not be able to impreſs upon the minds of the public. I ſuggeſted no new experiment, nor was any one wanting; the definitive laws I adduced, were contained in the works of the beſt writers on optics, and were ſufficient to evince the want of originality and improvement of his meniſcus ſhaped lens. In reſpect to the experiment by which he attempts to inforce a proof of an advantage in his ſpectacles, its value will be known by the following account of a repetition of it.

Experiment with a pair of periſcopic ſpectacles compared with a pair of the uſual conſtruction.

I am poſſeſſed of a pair of his periſcopic glaſſes, mounted in a ſingle ſteel frame, which coſt 10*s*. 6*d*. The glaſſes, I muſt obſerve, are different to his propoſed form, having of each, the inner ſide, or that next to the eye ſo little incurvated, that by any perſon but an optician, they would be called plano convexes. The focus is four inches, the ſame as uſed by Dr. Wollaſton in his experiment. In a ſimilar mounting, with double convex glaſſes of the ſame diameter and focus, I provided a pair of our own manufacture, and as ſold by us at 3*s.* 6*d.*" Theſe two pair of ſpectacles were attentively compared together, by myſelf and ſeveral judicious and impartial perſons, in the manner as ſtated by Dr. Wollaſton of his. The reſult was as follows.

Reſult. With the double convex lenſes. Extent of diſtinct viſion. Indiſtinct.

The convex glaſſes being applied as cloſe as poſſible to the eyes, with the frame attached to the head, the print of a large quarto page was viewed through them, at a diſtance for diſtinct viſion at their centres, the letters at the diſtance of about 25 lines, appeared quite diſtinct or well defined; giving the axis of the eyes a little obliquity to diſcriminate more lines, an indiſtinctneſs or confuſion of letters commenced, increaſing towards the extremity of ſight, and from the lateral aberration of the lenſes, the letters were tinged with the priſmatic colours. Keeping the head fixed in the ſame poſition, the periſcopic glaſſes were ſubſtituted. The extent of diſtinct letters without diſtortion was nearly as great, but the coloured letters were evidently nearer to the centre, and more numerous than by the other glaſſes. By inclining the axis of the eyes ſtill more than in the former caſe, or looking extremely aſquint through the glaſſes, a greater extent of lines was obſerved, but blended with colour and confuſion. The optic nerves felt a ſenſible irritation, evidently from the ſquinting poſition of the eyes, a refraction of many ſuperfluous rays, and the conſequent increaſed and unnatural magnitude of the images on the retina. The pain in the eyes mentioned by Dr. Wollaſton, muſt have ariſen only from this circumſtance, and not from the one he repreſented it to be. By a trial of the old meniſcus glaſs I before mentioned, which is of four inches focus, and correſponds with what he has a patent for, in compariſon with one of the above plano convexes, the view of letters was ſtill more extended, but illegible and with much colour, and like the others towards the extremity, of no ſort of uſe for the purpoſes of viſion. Now all this is conformable to the laws of optics, and manifeſts a property different to that advanced by Dr. Wollaſton.

With the periſcopic glaſſes, more colour.

Trial of another meniſcus.

Theſe ſeveral glaſſes are alſo at the public ſervice for inſpection in our ſhop in Holborn.

Concluding remarks.

By making the glaſſes of the above periſcopic ſpectacles nearly planos, Dr. Wollaſton's principle is deſtroyed, and my opinion evidently verified; that the nearer a meniſcus approaches to a plano, the more perfect it will be, as the ſpherical ſurface, for the ſame focus, is diminiſhed, and conſequently the aberration beſides; admitting that there were any advantage deſirable from a great obliquity of the axis of the eyes to thoſe of the meniſcus ſhaped ſpectacle glaſſes. I would aſk, for what reaſon has man his head moveable? was it not, that he ſhould place his eyes directly before the object to be viewed, and not ſubject himſelf to fallacious ideas of them, by an aukward and revolutionary ſquinting. From what I have advanced, I doubt not of the public deciſion; (from a fair compariſon of the two kinds of ſpectacles) in favour of the eſtabliſhed double convex ſpectacle-glaſſes, for

"*Magna eſt veritas et prævalebit.*"

I am, SIR,
Your reſpectful humble Servant,
W. JONES.

Holborn, April 10, 1804.

XI.

Experiments and Obſervations on the Change which the Air of the Atmoſphere undergoes by Reſpiration, particularly with Regard to the Abſorption of Nitrogen. In a Letter from ALEXANDER HENDERSON, M. D.

To Mr. NICHOLSON.

SIR,

Whether nitrogen be abſorbed in reſpiration.

I TAKE the liberty of communicating to you a brief detail of ſome experiments on reſpiration, which were undertaken chiefly with a view to determine the abſorption or non-abſorption of *nitrogen*, a point which has been hitherto much controverted, but which I flatter myſelf to have now ſufficiently aſcertained.

The gaſometer.

Theſe experiments were performed by means of a gaſometer, capable of containing about 2200 c. inches, and graduated ſo as to ſhew a difference of 2 c. inches. In breathing from this apparatus, the inconveniences from friction were very inconſiderable. Towards the end of the experiment, however,

ever, when the air in the gaſometer became vitiated, and the reſpirations fuller and quicker, ſome diſagreeable effects were experienced from the ſmallneſs of the breathing tube, and from it becoming, in ſome meaſure, choaked by the vapor from the lungs, which was condenſed in it.

Method of reſpiring.

After cloſing the noſtrils, and making a forced exhauſtion of the air in the lungs, a full inſpiration was made of the atmoſpherical air in the gaſometer, and this inſpiration as well as the ſubſequent expiration, was meaſured, by means of the graduated ſcale. The air of the gaſometer was then reſpired as long as poſſible, and till the oppreſſion about the cheſt became ſo great as to oblige the experimenter to deſiſt, and before cloſing the ſtopcock, and ſeparating the tube from the mouth, the magnitude of the laſt full inſpiration and expiration was accurately obſerved.

and general effect, as noted;

In all our experiments, the bulk of the atmoſpherical air was conſiderably diminiſhed by reſpiration, the quantity that diſappeared varying from five to eight c. inches per minute. This diminution, however, is partly to be attributed to the condenſation of the oxigen gas, when converted into carbonic acid gas, in which form its bulk is diminiſhed about one fourth. In order to obviate any objections that might apparently ariſe from the difference of the inſpirations and expirations at the commencement, and at the concluſion of the experiments, they were, as we have already mentioned, carefully noted; and if any difference did occur, it was deducted on the ſide where the preponderance took place.

Eudiometrical examination.

In proceeding to examine the chemical qualities of the air, conſiderable difficulties were at firſt experienced for want of an accurate eudiometer. Several trials were made with *nitrous gas*, which affords the moſt eaſy and expeditious mode of analyſing atmoſpherical air. But, independent of the inaccuracies which may ariſe from the number of veſſels it is neceſſary to have recourſe to in employing it, this teſt is liable to ſtill greater objections, from differences in its degree of purity, from its abſorbing a portion of nitrogen, and from its combining with a greater or leſs proportion of oxigen, according to the diameter of the veſſels employed, the length of time which it is allowed to remain in contact with atmoſpherical air, and the degree of agitation uſed in mixing them.

Nitrous gas exceptionable.

The

Seguin's eudiometer with phoſphorus objected to;

and alſo the impregnated ſulphate of Davy.

The eudiometer with *phoſphorus*, as recommended by Seguin, is by no means free from theſe diſadvantages. It is ſubject to the ſame inconveniences as the nitrous gas, with reſpect to the frequent change of veſſels; and, if we may credit the obſervations of V. Humbold, is liable to ſeveral ſources of fallacy from the different proportions of oxigen, which the phoſphorus abſorbs, and from its combining alſo with nitrogen. A portion of carbonic acid may alſo be generated, if the phoſphorus be not perfectly free from impurities The eudiometrical teſt, which has been lately recommended by Davy, viz. a ſolution of the *pale ſulphate of iron*, ſaturated with nitrous gas, is not altogether exempt from the inconveniences which attend the uſe of the latter ſubſtance, and has the additional objection, that a portion of nitrogen is generally diſengaged from the ſolution, after the abſorption of oxigen is completed.

Sulphuret of alkali or lime abſorb oxigen very ſlowly,

and are not completely amended in this reſpect by heat.

The *ſulphurets* of *alkalies* and *lime* have hitherto been regarded as among the moſt accurate teſts of the purity of atmoſpherical air. They, however, abſorb oxigen but ſlowly, and ſeveral days may elapſe, before the abſorption be completed. It is therefore, in general, neceſſary to have recourſe to complicated and uncertain calculations, in order to adjuſt the reſult of the experiment to the variations of the ſurrounding atmoſphere. To remedy this objection to their uſe, the application of heat was propoſed by Guyton, by which means a ſpeedy abſorption was effected; but, beſides, that ſome difference of the product may reſult from the method employed, the apparatus itſelf is objectionable, from its ſize and inequality of dimenſions

The apparatus of Dr. Hope affords great advantage,

Sulphuret of lime was uſed.

Luckily for the progreſ of eudiometrical ſcience, theſe obſtacles to the attainment of an accurate knowledge of the conſtituents of the atmoſphere, ſeem to be now, in a great meaſure, removed, by the invention of an apparatus by Dr. Hope, which unites the advantages of neatneſs and ſimplicity with thoſe of extreme accuracy and expedition, and of which you have already given a very full deſcription in your Journal*. In the trials which I have made with this inſtrument, the ſubſtance found to anſwer beſt as a teſt of the atmoſpherical air was the *ſulphuret of lime*, which is more readily decompoſed than the ſulphuret of potaſh, and is conſequently better adapted for the purpoſe of expedition. In moſt of the experiments, the abſorption was finiſhed in about twenty minutes, if ſufficient agitation had been employed. The only objection to which the above teſt may ſeem liable, is, that, according to ſome, it abſorbs a ſmall proportion of the nitrogen, as well as the oxigen of the atmoſphere. This opinion, however, does not appear to be well founded, for in ſeveral of our experiments, where the quantity of oxigen abſorbed appeared to be unuſually ſmall, and where the agitation was continued for a much longer time than neceſſary, no perceptible alteration in the bulk of the air was obſerved *.

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* Vol. VI. pages 61. 210.

It does not appear to abſorb nitrogen.

The method of proceeding in the analyſis of the air, was briefly as follows: after having aſcertained the purity of the atmoſpherical air by means of the eudiometer above-mentioned, and knowing the exact bulk of the air contained in the gaſometer, the total quantities of oxigen and hidrogen in it were calculated by a very ſimple proceſs. This air was then reſpired, and its diminution marked as has been already deſcribed. After reſpiration, a portion of it was introduced into the eudiometer, and its carbonic acid was abſorbed by means of lime water, (for which the above-mentioned inſtrument was found extremely convenient). Freed from the carbonic acid, the air was now ſubjected to the action of the ſulphuret of lime, and the relative quantity of nitrogen contained in it was thus diſcovered. Then, by deducting the quantity of carbonic acid, and of oxigen gas, contained in the air of reſpiration, from the total quantity that remained after reſpiration, we procured the proportion of nitrogen, which abſtracted from the total quantity before reſpiration, gave the proportion of nitrogen abſorbed. Thus, let a repreſent the original quantity of nitrogen; b the carbonic acid, and c the oxigen of the air of reſpiration, and M the bulk of the reſidual air; $M - b + c = n$, or reſidual nitrogen, and

$a - n = x$ of nitrogen abſorbed.

The proceſs. Previous examination of the air,

and after reſpiration.

Experiment I. June 16, 1803.

600 c. inches of atmoſpherical air were reſpired, for four minutes, at the temperature of 63°.

Exp. 1, 2, 3. The bulk of nitrogen abſorbed in reſpiration was about 1-40th of the whole, or about 1-32d of the nitrogen.

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* De Marti is of opinion that the hidrogenated ſulphurets abſorb nitrogen only when recently formed. Vide Journal de Phyſique. Tom. LIII. p. 176.

Before reſpiration, 100 parts contained of

Oxigen - - - -	.22
Nitrogen - - - -	.78

After reſpiration, quantity diminiſhed to 570 c. inches; 100 parts found to contain of

Carbonic acid - - -	.07
Oxigen - - - -	.14
Nitrogen - - - -	.79

$570-39.7+80=450.3.$

$468-450.3=17.7$ c. inches or quantity of nitrogen abſorbed.

Experiment II. June 18, 1803.

600 c. inches atmoſpherical air were reſpired, for four minutes, at the temperature of 64°.

Before reſpiration, 100 parts contained of

Oxigen - - - -	.22
Nitrogen - - - -	.78

After reſpiration, quantity diminiſhed to 570 c. inches, 100 parts found to conſiſt of

Carbonic acid - - -	.08
Oxigen - - - -	.12
Nitrogen - - - -	.80

Quantity of nitrogen abſorbed, therefore, =12 c. inches.

Experiment III. February 11, 1804.

1000 c. inches atmoſpherical air were reſpired for the ſpace of 4½ minutes, at the temperature of 57°. Barometer =28,78.

Before reſpiration, 100 parts contained

Oxigen - - - -	.22
Nitrogen - - - -	.78

After reſpiration, quantity diminiſhed to 962 c. inches.

After reſpiration, 100 parts contained

Carbonic acid - - -	.07½
Oxigen - - - -	.13
Nitrogen - - - -	.79½

Quantity of nitrogen abſorbed, therefore, =15.1 c. inches.

Comparative remarks on theſe experiments and thoſe of Davy.

Theſe different experiments, (which I have ſelected from among many others) agree in the general reſult, viz. that a portion of nitrogen is abſtracted from the atmoſpherical air, by the blood in its paſſage through the lungs, although the amount is ſomewhat leſs than has been ſtated by Davy, who makes it equal to about 5 c. inches per minute. Yet I am inclined to think, that this is more a difference in appearance than reality; for if we conſider that the moſt of Davy's experiments give the reſult of the changes produced on the air by a ſingle inſpiration, or by a ſmall number of reſpirations, while in the experiments juſt deſcribed, a large portion of air was breathed for a conſiderable length of time, ſo as to become, at laſt, unfit for the due performance of reſpiration; it is probable that the blood could no longer produce the ſame alterations in its properties, that took place when a purer atmoſphere was inſpired. It is alſo natural to ſuppoſe, that the quantity of air conſumed in reſpiration varies in different perſons, and in the ſame perſon at different times. An approximation to the truth, therefore, is all that we can expect to obtain in the determination of this queſtion; and we muſt reſt ſatisfied with the knowledge of the important fact, that nitrogen is abſorbed by the human body in reſpiration.

Dr. Thompſon alſo found ſulphuret of lime a very excellent teſt.

The ſtriking uniformity in the analyſis of the atmoſpherical air, by means of the ſulphuret of lime, occurred alſo, as I have with pleaſure obſerved, to Dr. Thompſon in the numerous trials which he has made on this ſubject; and furniſhes a moſt intereſting problem for the inveſtigation of the chemiſt and natural philoſopher.

I am, with much Reſpect,

Sir,

Your moſt obedient Servant,

AL. HENDERSON, M.D.

Edinburgh,
April 12, 1804.

Obſervations

XII.

Obſervations and Experiments tending to aſcertain the Cauſes of thoſe Irregularities in Chronometers, which are generated during conſiderable Intervals of Time, and have been aſcribed to external Cauſes. By Mr. JOHN HALEY, *Jun.*

To Mr. NICHOLSON.

SIR,

HAVING ſeen in the laſt number of your very uſeful Journal ſome remarks on chronometry, I have thought the accompanying obſervations might not be unacceptable. If you ſhould conſider them worthy of inſertion, I ſhall take the liberty of communicating the reſults of ſome experiments I am now making in the ſame line.

I am, SIR,

Your moſt obedient and humble Servant,

JOHN HALEY.

March 29, 1801.

25, Cleveland Street, Fitzroy Square.

Latent cauſes of error in time-pieces.

MY deſign in writing the following pages is to develope certain latent cauſes of error, and their mode of operation, which have been found in a greater or leſs degree to affect every machine hitherto conſtructed for meaſuring time.

If the errors of the machine be aſcribed to foreign cauſes, improvements will ceaſe.

It is certainly the part of a watch-maker to ſearch for and remove every mechanical cauſe that may affect the going of his machines, before he haſtily concludes that the cauſes of thoſe errors which are generated in a length of time, (and which have eſcaped his notice,) are foreign to its ſtructure; ſuch as the influence of the oil, the varying different denſity of the atmoſphere in which the wheels move, &c. For ſuch opinions when once adopted, muſt greatly tend to ſlacken his purſuits and put a ſtop to his hope of farther improvement.

The foreign cauſes are of little effect: For many machines have performed wonderfully well, while ſubject to action.

That theſe foreign cauſes do in fact produce errors deſerving much notice in the beſt adjuſted time-keepers, is probably an erroneous notion. For if ſo, how could ſo many of different conſtructions have been found to perform for very long periods of time, expoſed to all theſe cauſes, with a wonderful degree of accuracy. Indeed Mr. Cumming, in his treatiſe on clock and watch work, has ſuppoſed that the influence of the oil may be in ſome caſes even beneficial to the performance of the machine; though it muſt be allowed he there ſpeaks of ſuch as have no proviſion againſt the effects of heat and cold. And though Mr. Mudge, in ſome of his letters to his Excellency the Count de Bruhl does write, that when the air was moiſt in Devonſhire (where he made and tried his machines) they retarded their rates; yet the quantity of error produced by this ſuppoſed cauſe was ſo ſmall, that it gave him no uneaſineſs with regard to their fate. But the very different performance of machines on the ſame conſtruction, ſtrongly induce me to believe that moſt of the errors hitherto found in the beſt time-keepers have been produced by latent mechanical cauſes coexiſtent with the machines, or rather, with the times they were firſt put in motion.

Various opinions have been entertained by gentlemen of ſcientific and mechanical acquirements, and by artiſts upon this ſubject. One in particular has very generally prevailed, namely, that the errors have ariſen from inequality of power derived from the main ſpring, and the train of wheels. That very great errors will be produced by theſe cauſes, (if not removed by good workmanſhip,) muſt on all hands be admitted; but where the execution has been correct, the errors will be trifling indeed, and muſt always remain nearly the ſame. The late Mr. Arnold, on being aſked by a Committee of the Houſe of Commons his opinion of the Remontoue, ſaid, that it was only a help to bad workmanſhip.

Opinions reſpecting the cauſes of irregularities. Variation in the maintaining power, inconſiderable.

Remontoue condemned by *Arnold*,

Mr Harriſon's opinion of the Remontoire was different from that of Mr. Arnold; for he expected to arrive at perfection in his machine from introducing it, though by his method of application, he did not detach his eſcapement from the whole of the train of wheels. The invention was undoubtedly a great proof of his ſuperior ingenuity, and merited high praiſe. However, on finding the going of his watch not to anſwer his expectation, he attributed its irregularity to the thermometer not having its due effect; and aſſerted "that if it could properly be put into the balance, the watch would go within a few ſeconds a year," which aſſertion has ſince been proved erroneous.

but eſteemed by *Harriſon*.

Mr.

Mudge's Remontoire for winding up every vibration.

Mr. Mudge's comprehenſive mind ſuggeſted a Remontoire that ſhould be wound up at every vibration of the balance, and he attached it to a 'ſcapement which was certainly very ſuperior to Mr. Harriſon's, and for uniformity of excellence in its going, is more to be depended upon, in my opinion, than any other which has hitherto been applied to a portable machine. Mr. Mudge in the conſtruction of this 'ſcapement, had almoſt rendered his Remontoire uſeleſs with reſpect to one purpoſe for which he introduced it. His ſon, in a book entitled a Narration of Facts, (pages 46 and 47, in the margin) has publiſhed an extract from a manuſcript of his father's, wherein he writes as follows, "was I to make a watch myſelf upon this reaſoning, I ſhould not expect it to be by any means perfect, but I cannot help thinking, that I ſhould in the firſt eſſay get rid of ſo many, and ſo great errors, that the cauſe of thoſe that remain would be more comeatable than when blended as they are now, &c." I ſay, that he had, in the formation of his admirable 'ſcapement, nearly annihilated thoſe errors, the cauſe of which he propoſed to arrive at the knowledge of by the introduction of the Remontoire; it is much to be lamented that this great mechanic's powers failed him through age and infirmities, at the time when he had almoſt arrived at the knowledge of thoſe cauſes of error which he ſo earneſtly ſought after. And we may preſume from the ſuperiority of his penetration, that he would alſo have accompliſhed the means of removing them, and conſequently of giving to his machines all the perfection of which their principles were ſuſceptible.

He ſuppoſed it would detect unknown cauſes of error.

Mudge's watches accelerated their rates at firſt.

Dr. Maſkelyne's account of them.

It is obſervable that Mr. Mudge's watches, (which I conſider from their conſtruction as having the cauſes of error which I ſhall endeavour to develope, operating in them in a leſs degree than any other time-keepers,) in almoſt every inſtance accelerated their rates of going, while under trial at the Royal Obſervatory. The Rev. Doctor Maſkelyne has, I think, done mechanics an eſſential ſervice, by publiſhing his very judicious and accurate remarks upon their going while under his care. He obſerves, among other facts, "that the watches did generally accelerate their rates of going with ſometimes retarding them a little, that they accelerated their rates, leſs in the ſecond trial than in the firſt, and leaſt of all in the laſt trial, and that towards the latter part of that trial the watch *green* retarded its rate." If we carry our obſervations farther, we find, that, in their ſubſequent trial which took place at Mr. Dutton's, (the watches having had nothing done to them,) they both retarded their rates throughout. I will endeavour to ſhew why theſe watches did accelerate and retard their rates in this manner.

Great difficulty of aſcertaining the cauſes of error in Mudge's time-pieces, if in the train.

The errors of oppoſite tendency that did exiſt in theſe machines were ſo ſmall, and I may add, were ſuch a length of time in generating, that it became very difficult for Mr. Mudge to aſcertain their cauſe. Where was he to look for them? It was not to the 'ſcapement only his ſearch was to be confined, but he had alſo to traverſe through the whole train of wheels: For though he had applied his Remontoires beyond the train, yet the laſt wheel (if there was any inequality in the power derived from the main ſpring or train) would preſs ſometimes ſtronger, and at others weaker upon the pallets of the Remontoires; which pallets the balance in its vibrations (by means of the pins in the crank affixed to it) had alternately to unlock, before it could be impelled by the unbending of the Remontoire ſprings—therefore, as he ſuppoſed the cauſes were not determined to the 'ſcapement, he knew not where to fix them *.

Opinions of mechanics. 1. Of *Mudge*, that time-pieces err not from principle, but imperfect execution.

It was an opinion (in writing) of Mr. Mudge's, "that the ſimple principles of all watches are the ſame and perfect, the errors found in them are therefore not errors ariſing from the principles, but from imperfections, inſeparable from all mechanic operations."

2. *Arnold*: that his 'ſcapement wore leſs than other parts.

The late Mr. Arnold aſſerted before a Committee of the Houſe of Commons, that, though he put no oil to his 'ſcapement, yet it wore leſs than any other part that was ſubject to wear.

3. *Emery*: that *Mudge's* 'ſcapement would not eaſily wear.

Mr. Emery told the ſame Committee, that the 'ſcapement which he made for his Excellency the Count de Bruhl, after a model of Mr. Mudge's, was very difficult to execute, but when made, it would not eaſily wear out. It is a known fact, that this watch went with an unuſual degree of excellence.

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* For drawings and deſcriptions of Mr. Mudge's 'ſcapement, the detached 'ſcapement now in general uſe, and ſeveral others, ſee Philoſ. Journal, quarto ſeries II. p. 49.

4. General obſervation; that when the 'ſcapement wears there is no more regularity.

And ſtill more it is the obſervation of every watch-maker, when ſpeaking of the verge and horizontal 'ſcapements, that when once the verge or cylinder begins to wear, there is an end of all good performance, and that the going of the watch becomes from that period totally incorrect.

Inference: that the errors in the beſt time-pieces are thus cauſed.

Theſe ſeveral teſtimonies tend to induce a belief, that the errors which ſtill remain in the beſt time-keepers, are produced chiefly by this cauſe; and this cauſe muſt exiſt from the moment the machine is firſt put in motion, although the errors will not manifeſt themſelves to any conſiderable extent, (particularly if the materials are of the beſt kind,) until the machine has been going for ſome time.

Diſtinct ſtatement: that the alterations of rate are cauſed by wear in the 'ſcapement.

I therefore ſubmit it as a fundamental principle, that the principal cauſe of the errors found in the beſt adjuſted time-keepers, conſiſt in the wearing of the different parts of the 'ſcapement; that ſo long as by the act of wearing the relative proportions of its parts are preſerved, that errors of contrary kinds compenſate each other in the general action, the machine will go correctly; but ſo ſoon as thoſe ratios of the parts are altered by wearing, the watch will go either too faſt or too ſlow.

Short deſcription of the uſual detached ſcapement. 1. Scape wheel, 2. (face of) detent, 3. paſſing ſpring, 4 unlocking pallet, 5. impulſe pallet.

The ſcape wheel of what is termed the detached eſcapement, ſay of the late Mr. Arnold, or any other of them in uſe, (for their principles are the ſame, and differ only in modification,) is looked upon a jewel in the detent. The detent has a ſmall ſpring faſtened to it, which reaches conſiderably beyond the locking jewel. Some watch-makers have given the name of paſſing ſpring to it, becauſe, the little pallet in one vibration of the balance paſſes it without diſturbing the detent, and in the next vibration, the ſame pallet by laying hold of its point, lifts the detent out of the wheel, which immediately impels the balance by means of the great or impulſe pallet*.

Conſequences of wear. 1. If the wheel (point) ſhould wear, it will *ſooner* eſcape and ſtrike the pallet.

To ſimplify our deductions, let it be ſuppoſed that the point of the paſſing ſpring does not wear at all, and I think it will appear to every perſon acquainted with the 'ſcapement, that if the points of the teeth of the wheel ſhould wear, the watch muſt go faſter. For the wheel muſt then eſcape the locking jewel of the detent ſooner, and will neceſſarily impel the balance ſooner alſo; conſequently each impulſe given to the balance muſt be began quicker and quicker, as the points of the teeth of the 'ſcape wheel wear more and more. But on the contrary, let us now ſuppoſe that the points of the teeth do not wear, and the paſſing ſpring wears; in this caſe, as the balance in its vibration will have a greater ſpace or portion of a circle to move, before it can arrive at the ſpring, and unlock the wheel, in order to receive the impulſe, every ſuch impulſe will be made later and later, and the watch muſt neceſſarily go ſlower and ſlower as the point of that ſpring wears away more and more. There is another part in this 'ſcapement, namely, the ſtriking face of the tooth, in which, if wear takes place, the tendency I preſume will be to loſe, in conſequence of the wheel acquiring a greater drop, or having farther to go before it can overtake and ſtrike the pallet. This part of the tooth I here ſuppoſed to wear, and which firſt comes into contact with the pallet, is not the point, but a little nearer the centre of the wheel; and as the wheel follows the pallet, it muſt be ſome portion of time longer before it can arrive to begin the impulſe. In the above example I have ſuppoſed the detent and both pallets to be jewelled: if they were not, the errors would be greater.

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* Philoſ. Journal. Quarto II. 54.

If the paſſing ſpring wear, the unlocking, and conſequently the impulſe will be *later*.

If the face of the teeth ſhould wear, the drop will be more and the impulſe *later*.

Now we find that, according to the above reaſoning, although there are two parts of the 'ſcapement in which, if wear takes place, the tendency will be to loſe; leaving out of the queſtion the oil becoming glutinous, the ſprings loſing their elaſtic force, &c. and only one part, in which if wearing takes place the tendency will be to gain, yet, we find in moſt machines the tendency to gain is generally predominant. I will ſhew why I think the teeth of the wheel wear more than either of the other two parts: there is not only friction, but percuſſion alſo takes place in the three different parts of the 'ſcapement before-mentioned; the little roller or pallet that lifts the detent out of the wheel ſtrikes againſt the point of the paſſing ſpring on coming into contact in order to unlock the wheel; in the next place the wheel ſtrikes againſt the pallet when it begins the impulſe, and laſtly, the wheel again ſtrikes againſt the jewel of the detent when it locks upon it: I will not attempt to determine which of the two former percuſſions is the greateſt, or which will occaſion the greateſt retardation of rate, but will ſay, that the laſt is greater than either of the other two by much, and in general we find that the wheel is locked upon

Thus there are two kinds of wear which tend to produce loſs, and one to produce gain.

—but the gain predominates,

—and this becauſe the ſtroke of the wheel in locking is much greater, that thoſe of the unlocking and the impulſe.

that part of the face of the tooth neareſt the point; now as repeated ſtrokes will wear as well as friction, I take it that the wheel wears more from this cauſe (particularly where the execution has been favourable for it) than either of the other two parts, and therefore moſt time-keepers made upon this conſtruction have a greater tendency to gain than they have to loſe.

The advantage of clocks or machines with ſhort vibrations deduced from the ſmaller percuſſion.

If it ſhould be admitted that percuſſion will wear in any material degree, I ſubmit that it will lead to the diſcovering why clocks whoſe pendulums make the ſhorteſt vibrations go better than others, and alſo the cauſe of the great diſparity there is between the going of the common verge watch and that of table and long eight-day clocks; in both clocks and watches conſtructed with two pallets upon the recoiling principle, each pallet, with the accumulated force it has obtained by the vibration of the balance or pendulum, meets the wheel which is coming a contrary way when it comes into contact, and a percuſſion takes place; therefore the longer the vibration of the pendulum or balance, the greater muſt this percuſſion be, and conſequently the wear alſo: hence may be ſuppoſed one reaſon why ſpring or table clocks that make ſhorter vibrations go better than verge watches, and long clocks, which make ſhorter vibrations ſtill, go better than either. I readily admit there is one, and perhaps many weighty objections to this reaſoning, viz. that ſome clocks upon the recoiling principle are found to go as well or even better than others on the principle of the dead beat, though the execution in each has been of equal excellence. For an anſwer to this objection, I refer the reader to Mr. Cumming's elaborate treatiſe upon clock making; who in ſome part of the work, aſſerts, the reaſon to be, that the plane of action of the pallet does not ſufficiently ſubtend the angle of vibration; by which means the power applied is adminiſtered too ſuddenly in the dead beat; to which I add that the percuſſion is alſo greater in this caſe in the dead beat than in the recoil, although the pallet does not meet the wheel in oppoſition, and the wearing of the teeth of the wheel may be increaſed by that means. There will be among many others one very ſerious objection to my ſuppoſition that the wearing of the wheel can produce error in a long pendulum clock, viz. the power of the pendulum being ſo much ſuperior to the motive force, it cannot be affected by it; to which I ſhall only obſerve that the reſults in practice are ſometimes very different from theoretical concluſions.

Some objections conſidered.

Whether gain be occaſioned by ſhortened vibrations.

The mode of operation by which friction is ſaid to accelerate the rate of machines, is thus pointed out by the trade in general; viz. that whenever it takes place by the action of the wheel upon the pallet or pallets; (in watches or clocks conſtructed with one or two pallets), or in the verge holes by the action of pivots therein, it impedes the motion of the balance or pendulum, ſhortens its vibrations, and in conſequence the machine goes faſter: how does this agree with Mr. Harriſon's opinion? which was, that large arcs are naturally performed in leſs time than ſmall ones. Mr. Cumming, in the work of his before alluded to, has intimated, that nothing concluſive can be drawn from different arcs, becauſe they are effects and not cauſes of error.

An eſcapement completely detached.

In the Repertory of Arts and Manufactures, No. 33, in the year 1796 or 7, there is a deſcription of a Remontoire 'ſcapement, which is completely detached from the train of wheels. The errors of this machine muſt be determined to the 'ſcapement alone, becauſe no errors in the going can poſſibly ariſe in any other part of the watch. For if any additional power, however great, be applied to the train, the arcs of the balance will not be in the leaſt affected by it: in the opinion of all or moſt who underſtood its principles, its going muſt be more perfect than that of any machine before invented.

In this the wear on the locking is greater becauſe the action is on a ſingle face.

In the following remarks upon that 'ſcapement, I ſhall call, what is in the before-mentioned deſcription of it termed a ſnail pallet, a locking pallet. In the commonly termed detached 'ſcapement, every tooth in the wheel is locked in ſucceſſion upon the jewel of the detent, and the effect ariſing from wearing of the teeth does not very ſoon ſhew itſelf, there being 12,15 or more (at pleaſure) in number, but in this remontoire 'ſcapement before every impulſe there is only this individual locking pallet to ſupply the place of a wheel, (if I may ſo expreſs it) the wear therefore muſt be 12 or 15 times as great, and its effect muſt manifeſt itſelf very ſoon.

Account of the conſtruction and early performance of ſeveral of theſe.

In the years 98 and 9, I executed four or five of theſe 'ſcapements, and tried the going of each. They all went in the ſame manner, viz. for the firſt twenty-four or thirty hours the machine would vary nothing; the day following it would gain one or two ſeconds, the next day ſeven or eight, and on the fourth or fifth day it would gain 30 or 40. I obſerved, the more the machine gained, the larger were the arcs of the balance;

lance; if the machine was ſet a-going without oil being applied to the locking pallet, it would not go many days before it would ſtop, and if ſet in motion the balance would perform only three or four vibrations, when it would come to reſt again: in this ſtate, if oil was applied to the locking pallet, it would ſet a-going more lively than ever, the balance making conſiderably larger arcs, and the machine gaining upon its former rate in a very ſurprizing manner.

Explanation of the irregularities;—from wear or change in the parts, and not from friction only.

Should it here be enquired, how were theſe effects produced? I think it would be anſwering in a vague and indefinite manner to ſay merely,—that it was occaſioned by friction; for the mode by which it operated is certainly neceſſary to be pointed out: I ſuppoſe that by friction the point of the locking pallet (which was made of ſteel) wore ſhorter, by which means it locked ſhallower upon the jewel in the detent, therefore the balance, unlocking the Remontoire ſooner, the impulſe to the verge was given quicker as the point of the locking pallet became ſhorter by wear; alſo, the ſhallower the locking pallet locked upon the jewel of the detent, the leſs was the reſiſtance to the balance in unlocking it, and the drop upon the impulſe pallet became leſs, therefore the arcs of the balance became larger in proportion as the locking pallet wore ſhorter: I can never believe that the above effects, viz. acceleration of rate and large arcs were produced by friction abſtractedly; for had it been ſo, they would have ceaſed the moment oil was applied to the locking pallet, inſtead of which the effects became more apparent.

Remedy. An intermediate wheel was introduced, to render the locking faces more numerous.

It was not till after various examinations of the 'ſcapement alluded to, and making two or three others of the ſame kind, that I formed the foregoing concluſions reſpecting the cauſe of the error that had puzzled me; when I had fixed upon the cauſe in my own mind, I determined upon a remedy, which was, by applying an intermediate wheel between the remontoire and the balance, inſtead of the locking pallet; I contrived to fix a ſhort weak jewelled detent upon the remontoire axis, which moved round with it and locked upon the intermediate wheel, which had twenty-four teeth; as the locking pallet had before locked upon the detent, I placed alſo a detent upon the other ſide of the intermediate wheel, to lock it, and by a particular contrivance made the intermediate wheel to ſhift one tooth at every vibration of the balance; after which alteration, the former effects totally ceaſed; in order to make the intermediate wheel (which was looſe) ſhift one tooth at every vibration, I made the detent which locked it ſtrong, and inſerted a long jewel into it, having an inclined plane that filled the ſpace between each two teeth that always kept the wheel in a certain poſition, the conſequence of applying the ſtrong detent was, that the point of the paſſing ſpring (or in other words the unlocking part) wore very faſt, and occaſioned a very conſiderable retardation of rate, which continued without intermiſſion until the watch ſtopped from the point of the paſſing ſpring wearing ſo much, that the balance could not unlock it. I muſt obſerve, that the arcs of the balance were in this caſe not perceptibly altered, and the retardation not nearly ſo great as the acceleration in the former. I afterwards, by another contrivance, which it would be difficult to explain, remedied the evil attendant upon a ſtrong detent, and produced an eſcapement perfectly to my wiſhes, that had no propenſity either to accelerate or retard its rate.

It ſucceeded.—Strong detent to ſhift the wheel

—produced wear in the paſſing ſpring;

—and retardation.

Remedy.

It is clear to me, that friction or wearing will, in ſome caſes, make a machine accelerate, and in others retard its rate of going, therefore I preſume if we could hit upon ſome method of preſerving the ratios of the detached 'ſcapement permanent, by giving the different parts a durability, they have not hitherto poſſeſſed, we might produce machines of this ſimple conſtruction that ſhould retain an equal rate of going for long periods of time, and not look to chance for their ſucceſs; if the actions of the 'ſcapement were unalterable (which I imagine might be made by jewelling) we ſhould then have only the influence of the oil to oppoſe our ſucceſs, which by becoming glutinous, will, I think occaſion, if any change, a retardation of rate, yet if it be good, the effect, I ſuppoſe, will not be very great in a moderate length of time.

The ſimple detached 'ſcapement would perform well for long periods of time if preſerved from wear.

Having been on the coaſt of Coromandel and Ceylon, and other parts of the Eaſt Indies, for a ſeries of eight years, I had there an opportunity of trying an oil to watches, which artiſts here are unacquainted with; being in camp in the Myſore country, in the years 1791 and 2, for a period of about eighteen months, I could ſcarcely procure a drop of olive oil that was not rancid; I therefore ſubſtituted in room of it what is there called cocoa nut oil, which retains its fluidity to the laſt. I never knew an inſtance of its becoming glutinous, nor

Cocoa nut oil recommended for time pieces. —

does it evaporate ſoon; I have found it in jewelled, verge, and horizontal watches, in that warm climate, after a period of three years, in a very fluid ſtate, and in ſufficient quantity. That oil may be eaſily procured here by breaking the ſhell of the cocoa nut, drying the nut in a very gentle manner, and afterwards expreſſing the oil from it, which is yielded in a great quantity; it aſſumes a concrete form in a very moderate degree of cold, but how far that may be a good objection againſt its uſe in this climate remains to be determined.

Mudge's firſt watch ſtopped for want of oil on the pallets.

I will conclude theſe obſervations, after a few remarks upon the ſtopping of Mr. Mudge's firſt watch, while under trial at the Royal Obſervatory; Mr. Mudge found, after a good deal of pains taken in inveſtigating the cauſe, that it was for want of oil being put to the pallets (or locking part) of his 'ſcapement: I think this caſe ſo parallel to that of Mr. Haley's machines ſtopping from the ſame cauſe, that it tends in ſome meaſure to prove the truth of what I have advanced: Mr. Mudge had thought when he finiſhed this machine, that it was altogether unneceſſary to apply oil to this part, as he mentions in a letter to his excellency the Count de Bruhl, upon the ſubject of its ſtopping; and in the ſame letter he writes to the following effect, that when he had applied oil to the pallets, the watch went as well as before, or, as it did when firſt ſet a-going; that the arcs of vibration had fallen off but a few degrees, yet the watch went ten ſeconds a day faſter than the laſt-mentioned rate, and from any thing that ſuggeſted itſelf to him, he could not ſee why it ſhould not have gone rather ſlower than faſter through length of time. I will venture to ſuppoſe the following to have been the cauſe of the machine going faſter, and of the balance meaſuring ſmaller arcs in conſequence.

Effect when oil was applied.

Explanation: The teeth of the wheel were ſhorter; eſcaped ſooner, and (qu.) —

The points of the teeth of his 'ſcape wheel had become ſhorter * by conſtant wear, however ſmall, and eſcaped the pallets (or rather hooks of the pallets, as they have been termed) ſooner; therefore the oppoſite pallets which impelled the crank gave the impulſe ſooner to the balance, and in my mind, the watch may be ſuppoſed to have accelerated its rate from this cauſe; on the other hand, by the wearing of the teeth, the wheel did not wind up the remontoire ſprings quite ſo

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* Refer to the figure, Philoſ. Journ. 4to ſeries, Vol. II. p. 49.

high, therefore there aroſe in ſome degree an impediment to the balance, which might occaſion the arcs to be ſhortened, becauſe, before the wheel was ſet at liberty, or unlocked from one pallet, in order to wind up the other remontoire ſpring, the balance (by means of the crank affixed to it) had alternately to wind up each remontoire ſpring a little higher, and in ſhort, ſo much higher than the wheel had before wound it, as the momentum of the balance was equal to; therefore, as I ſaid before, when the teeth of the wheel became ſhorter by wear, they would not wind up the remontoire ſprings quite ſo high; in which caſe the crank would engage the pallets on the oppoſite ſide of the remontoire axis a little ſooner, by which means the balance would receive a check in its vibrations, and the arcs would certainly fall off. Theſe, which I have mentioned, are the only reaſons which have occurred to me to ſhew why the remontoire machine I made ſhould have its arcs of vibration increaſed when the watch went faſter, and, on the contrary, Mr. Mudge's ſhould have its arcs ſhortened when it went the ſame way.*

—and the remontoire ſprings would be leſs wound up: whence the balance would have more to do alone.

As to Mr. Mudge's watches, *green* and *blue* retarding their rates while under trial at Mr. Dutton's, after having gone through their public trials at the Royal Obſervatory, it ſhould be remembered they had not been cleaned, and that the oil being thicker, muſt have had a ſhare in producing that effect, and perhaps a principal one; for as to friction in the impulſe or unlocking part, there was none, it was totally annihilated, by the verge and remontoire axis moving on the ſame centre; if any wear could take place there, it muſt be by percuſſion and preſſure only: likewiſe it may be conſidered, that the teeth of the wheel (or locking part) having been blunted by wear with former going, would have larger ſurfaces to reſiſt the wear and its accelerating effect during this trial; beſides, it will be readily believed by thoſe acquainted with Mr. Mudge's ſcapement, that the wearing of his wheel and its pernicious effect, cannot be ſo great as in that of the detached 'ſcapement.

The retardation of Mudge's other time pieces aſcribed to inſpiſſation of the oil.

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* It appears to me, that if the impelling pallet came to a ſtop before the inſtant of unlocking, no other than the laſt effect, in Mudge's machine, would take place; except that the reſiſtance to unlocking might become leſs. Would the crank and levers wear and diminiſh the effect of the acting pallet? —N.

XIII.

A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-Snake and other American Serpents. By BENJAMIN SMITH BARTON, M. D. *From the American Tranſactions,* Vol. IV.

(Continued from p. 285 of Vol. VII.)

Experiment of faſcination by a ſnake.

THE facts which came under the notice of Mr. Voſmaër, at the Hague, are curious, and deſerve to be mentioned. But they do not appear to me to be proofs of the exiſtence of an infectious or mephitic vapour proceeding from the mouth of the rattle-ſnake. I am not at all ſurpriſed that the birds and mice that were put into the cage, along with this reptile, ſhould exhibit the motions which were obſerved by the Dutch naturaliſt. When the little animals ſquatted down in a corner of the cage, they were, moſt probably, impelled by the inſtinct of fear, which is ſo powerful, and ſo extenſive, in the vaſt family of animals. When they run towards the ſerpent, it may have been fear that actuated them.

Perhaps partially obſerved.

In conducting a ſeries of experiments, it is ever a matter of importance, that the mind of the experimentaliſt ſhould be free from the dominion of prejudice and ſyſtem. Perhaps, facts are never related in all their unadulterated purity except by thoſe, who, intent upon the diſcovery of truth, keep ſyſtem at a diſtance, regardleſs of its claims. The ſtrong democracy of facts ſhould exert its wholeſome ſway. I cannot help thinking, that if Mr. Voſmaër had diſbelived the faſcinating faculty of ſerpents, the concluſions which he would have drawn from his experiments, juſt mentioned, would have been ſomewhat different. But of this I cannot be certain, and, therefore, I ſhall not avail myſelf of the ſuppoſition.

Other facts which contradict the notion.

Some experiments which have been made in this city, do not accord with thoſe of Mr. Voſmaër. The birds, which were put into the cage that contained the rattle-ſnake, flew or ran from the reptile, as though they were ſenſible of the danger to which they were expoſed. The ſnake made many attempts to catch the birds, but could ſeldom ſucceed. When a dead bird was thrown into the cage, the ſnake devoured it immediately. He ſoon caught and devoured a living mole, an animal much more ſluggiſh than the bird. A few days ſince, I had an opportunity of obſerving the following circumſtance. A ſmall bird, our ſnow-bird*, had been put into a cage containing a large rattle-ſnake. The little animal had been thus impriſoned for ſeveral hours, when I firſt ſaw it. It exhibited no ſigns of fear, but hopped about from the floor of the cage to its rooſt, and frequently flew and ſat upon the ſnake's back. Its chirp was no ways tremulous; but perfectly natural: it ate the ſeeds which were put into the cage, and by its whole actions, I think, moſt evidently demonſtrated, that its ſituation was not uneaſy.

No mephitic vapour was emitted by the ſnake in the experiment,

I do not relate this latter fact with any intention to diſprove the notion, that the rattle-ſnake poſſeſſes the faculty of charming. For the obſervation was made on the ſeventeenth of laſt month, which is ſomewhat earlier than the time when our ſnakes uſually come out of their dens. The ſnake, too, which was the ſubject of the experiment, appeared to be very languid, and had not eaten any thing for a conſiderable time. We ought not, therefore, to ſuppoſe him poſſeſſed of the faſcinating faculty at this period; ſince, I preſume, that this faculty, did it exiſt at all, is ſubſervient to the purpoſe of procuring the reptile its food. The fact is, perhaps, valuable in another point of view. It ſeems to ſhow, it does ſhow, that the mephitic vapour proceeding from the rattle-ſnake, allowing that ſuch a vapour really exiſted, was, in no reſpect, injurious to the bird.

nor in the wood at large.

If the mephitic vapour of the rattle-ſnake were productive of the effects attributed to it by Mr. de la Cépède, and other writers; and, eſpecially, if this vapour extended its influence to animals ſituated at a conſiderable diſtance from the reptile, the atmoſphere of the rattle-ſnake would often be a kind of Avernus, which many animals would avoid, and which would generally occaſion the ſickneſs or death of thoſe that were ſo unfortunate as to come within its ſphere. But how different is the caſe! The abodes of the rattle-ſnake are the favourite haunts of frogs, and many ſpecies of birds, which often paſs the ſeaſons of their amours and generation in clouds of mephitiſm; uninjured, and undeſtroyed. How often has the rattle-ſnake been known to continue, for days, at the bottom of a tree, or even a ſmall buſh, upon the branches of which the thruſh or cat-bird are rearing their young! This would be a ſuitable ſituation for the mephitic vapour to exert its noxious influence; but, in our woods, ſuch influence has never been perceived.

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* The Emberiza hyemalis of Linnæus.

Other inſtances in proof.

Birds of the eagle and the hawk kind have been ſeen to ſoar, for a conſiderable time, above the ſpot occupied by a rattle-ſnake, and at length to dart upon the reptile, and carry it to their young. Neither the parent-bird nor its young ones, have ever been known to receive any injury from the ſnake's vapour. Poſſibly it may be ſaid, this vapour was diſſipated, or greatly diluted, in paſſing through the air.

Whether other animals emit mephitic vapour.

A mephitic, or fetid, vapour emanates from the bodies of many animals, beſides the rattle-ſnake; from the opoſſum *, and the pole-cat †, for inſtance. The vapour of theſe quadrupeds would be as likely to affect birds, &c. with aſphyxy, as that of the rattle-ſnake. And poſſibly it does. There is, certainly, one thing in favour of the ſuppoſition. The opoſſum, in particular, is noted for his cunning in catching birds.

I ſhall conclude this part of my memoir by obſerving, that the odour of the rattle-ſnake is ſaid to be agreeable to ſome perſons.

Queſtion, if the agitated animals may not have been already bitten.

Mr. de la Cépède's ſecond mode of explanation is much more plauſible. I have already obſerved ‡, that it was the ſyſtem of Sir Hans Sloane, who affected to ground it upon experiments. It is adopted by the author of the well-written account of de la Cépède's *Natural Hiſtory of Serpents,* in the *Monthly Review* §.

Mr. de la Cépède preſumes that, "for the moſt part, when a bird, a ſquirrel, &c. has been ſeen precipitating itſelf from the top of a tree, into the jaws of a rattle-ſnake, it had been already bitten; and that its whole conduct, ſuch as its crying, its agitation, its leaping from branch to branch, &c. are all effects induced by the violent operation of the poiſon, thrown into its body, by the reptile.

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* Didelphis Opoſſum. † Viverra Putorius.

‡ See pages 30 & 34, *note.*

§ Appendix to the ſecond volume of the Monthly Review enlarged. p. 511.

Moſt probably not:

An attention to facts conſtrains me to reject this attempt towards a ſolution of the queſtion, which I am conſidering. I ſhall arrange my chiefeſt objections under two heads.

—becauſe the effects are more ſpeedy and very different from what are related of faſcination.

Firſt. We are pretty well acquainted with the moſt prominent effects produced by the poiſon of the rattle-ſnake, in various ſpecies of animals. It muſt be admitted, that there is a conſiderable variety in theſe effects, and a great difference in the ſtrength of theſe effects. In one animal, the poiſon produces an high degree of inflammatory action in the ſyſtem; in another, the moſt ſtriking primary effect is a ſomnolency, or drowſineſs. In one animal, the poiſon does not produce any obvious effect upon the ſyſtem for many minutes; in another the effects are almoſt inſtantaneous *. But in almoſt every inſtance in which the poiſon of the rattle-ſnake has been ſucceſsfully thrown into the body of an animal, there enſue a ſet of ſymptoms, very different from the actions of birds and ſquirrels when under the ſuppoſed faſcinating influence of the ſerpent-kind. It is not neceſſary to detail, in this place, theſe various ſymptoms, becauſe I have already done it in a paper, which is printed in the third volume of the *Tranſactions* of our Society †, and becauſe theſe ſymptoms cannot be unknown to the members of the Society. It will be ſufficient to obſerve, that two of the moſt univerſal effects of the poiſon of the rattle-ſnake, I mean the extreme debility and the giddineſs, which commonly almoſt immediately ſucceed the bite, will preclude the poſſibility of a ſquirrel's, or a bird's, dancing from branch to branch, flying about, and running to and from the ſerpent, for a conſiderable time, before it becomes a prey to its enemy. Beſides, the farce of faſcination is often kept up for a much longer term of time than any ſmall animals are known to live after a ſucceſsful bite by the rattle-ſnake. But, perhaps, it may be ſaid, that the rattle-ſnake, like ſome of our waſps, knows how to inject into the animal, which he means to devour, any given quantity of his ſubtile poiſon. Here, the analogy will not apply: but I have not time to point out the various inſtances in which its failure is conſpicuous.

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* A ſmall dog that was bitten in the ſide by a large rattle-ſnake, reeled about, and expired, ſeemingly ſuffocated, in two minutes. This was in the month of Auguſt.

† No. xi. p. 110 and 111.

The ſyſtem of faſcination implies that the animal may be relieved and eſcape.

Kalm mentions a well-known fact, which will be admitted to have conſiderable weight in deſtroying the force of this part of Mr. de la Cépède's ſyſtem. "The ſquirrel being upon the point of running into the ſnake's mouth, the ſpectators have not been able to let it come to that pitch, but killed the ſnake, and as ſoon as it had got a mortal blow, the ſquirrel or bird deſtined for deſtruction, flew away, and left off their moanful note, as if they had broke looſe from a net. Some ſay, that if they only touched the ſnake, ſo as to draw off its attention from the ſquirrel; it went off quickly, not ſtopping till it had got to a great diſtance." "Why," continues our author, "do the ſquirrels or birds go away ſo ſuddenly and why no ſooner? If they had been poiſoned or bitten by the ſnake before, ſo as not to be able to get from the tree, and to be forced to approach the ſnake always more and more, they could however not get new ſtrength by the ſnake being killed or diverted *."

(To be continued.)

SCIENTIFIC NEWS.

Very extenſive Table of Squares.

ABOUT three years ago, a large quantity of mathematical papers were brought to Oxford, which had belonged to Mr. Councer, an attorney at Bloxam, in the north of Oxfordſhire. They conſiſted chiefly of mere tranſcripts and collections from different publications: But there were two works among them, which muſt be excepted from this deſcription, and which prove Mr. C. to have been a man of ſingular induſtry.

The one was a table of ſines and tangents for every ſecond of the quadrant. It is probable, that this table was completed before the year 1760; but it would have been of no value, even if it had not been ſuperſeded by Mr. Taylor's publication. For Mr. C. ſeems, in this inſtance, to have been ignorant of the true method of calculating, and to have only interpolated ſome old tables, without allowing for the variation of the differences. The other work is really valuable, it is a table of ſquares and cubes to a much greater extent than that, which was publiſhed by the Board of Longitude in 1781, under the care of Dr. Hutton*. This table was calculated for the ſquares of all numbers from 1 to 128540, and for the cubes of all from 1 to 26560. But there is an interval in the ſquares from 28261 to 29061. It appears to have been originally left in the manuſcript, although it is impoſſible to conjecture the reaſon of the omiſſion. The grocer, who had purchaſed the papers, had begun to tear this table before it could be reſcued out of his hands; but as he had not got beyond the number 6000, this circumſtance is comparatively of little conſequence. In all other reſpects the work is complete. It is written in a clear hand, and (having been examined in ſeveral hundred places) appears to poſſeſs a degree of correctneſs, which may in general be depended upon.

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* Travels into North-America, &c. vol. ii. p. 209 & 210. It will be eaſy to diſcover what part of Kalm's reaſoning, in the above quotation I admit.

Among the papers were propoſals for printing this table; they are dated 1758. There were, likewiſe, two ſheets of an introduction to them, printed in 1761. At this diſtance of time, it is impoſſible to aſcertain the reaſon, which prevented the publication; but it is not improbable that Mr. C. might have been ſtopped by the great expence of the undertaking. Had Dr. Hutton been in poſſeſſion of this manuſcript, it might have ſaved much of his valuable time, and if any man of ſcience ſhould be employed in continuing his work, it will be uſeful to him to know that Mr. Councer's has been preſerved.

P. S. Mr. Councer had, likewiſe, calculated all the powers from the 1ſt to the 13th (incluſive) of the ſeries of numbers from 1 to 20. And he has continued this, for the nine digits, as far as the 26th power. *S. R.*

⁂ I received the preceding notice from the reſpectable gentleman whoſe initials are ſubjoined; and am enabled to give ſuch farther information as may conduce to render the papers of public utility. *W. N.*

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* Dr. Hutton's table is for the ſquares of all numbers from 1 to 25000, and the cubes of all from 1 to 10000.

On Phoſphoric Rings; from a Correſpondent: R. B.

Phoſphoric rings.

I DO not know that any one has explained the phenomenon of thoſe denſe cloudy rings which are produced when phoſphorated hydrogen ariſes through water, and explodes in the air; an effect ſimilar to what is ſometimes ſeen on the firing of cannon. Upon cloſe inſpection the fact appears to take place thus. The bubble of hydrogen, containing phoſphorus in ſolution, riſes and takes fire. Phoſphoric acid is ſeparated in the form of white ſmoke; through which denſe maſs the expanſive action is directed upwards. A fluid opaque ring, denſer than the atmoſphere is thus formed, the inner ſurface of which is made to aſcend by the rapid ſtroke, while the external part has received little or no impulſe. The natural conſequence is a quick rotation of the ring, from within outwards, which ſhews itſelf to the ſight, and ſeems, in ſome manner or another, as if it kept the parts together.

How to meaſure the Contents of any Pipe by a very ready Method.

Contents of any pipe in pounds or gallons.

MY ingenious correſpondent, Mr. Woolf, ſometime ago gave me a ready method of meaſuring the contents of a pipe, as follows:

Square the diameter in inches, and the product will be the number of pounds of water in every yard length of the pipe. Or if the laſt figure be cut off or conſidered as a decimal, the remaining figures will give the ale gallons in the yard.

Dr. Winterbottoms Machine for clearing Roads from Mud.

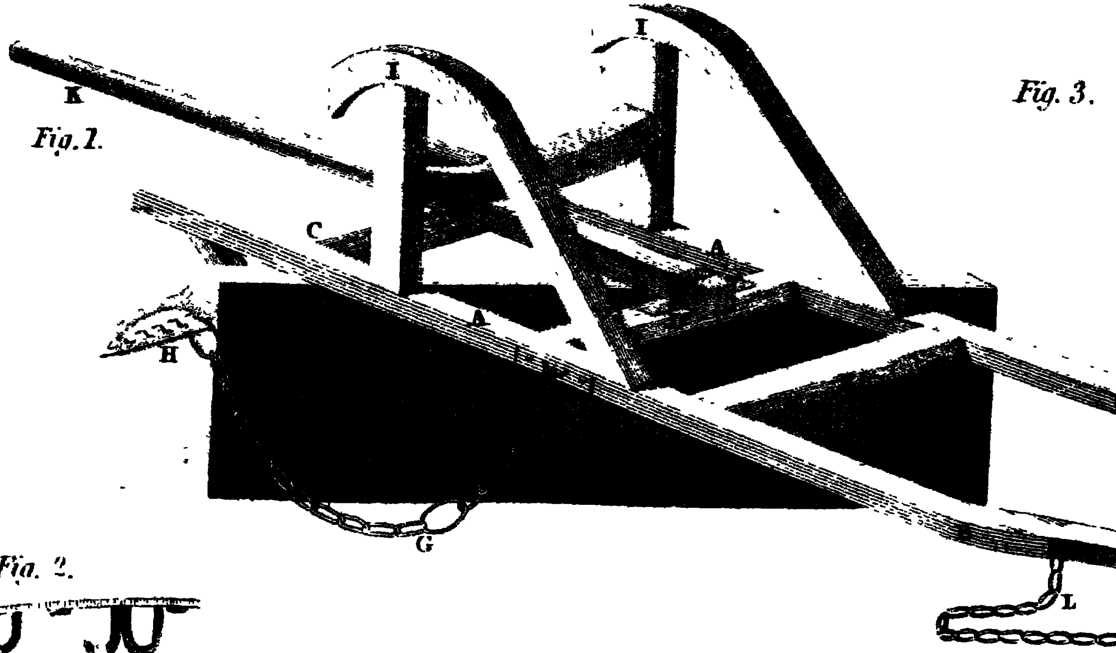


Fig. 1.

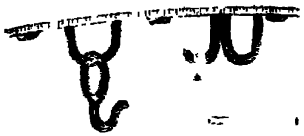


Fig. 2.

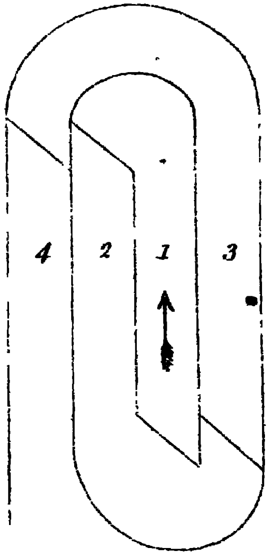
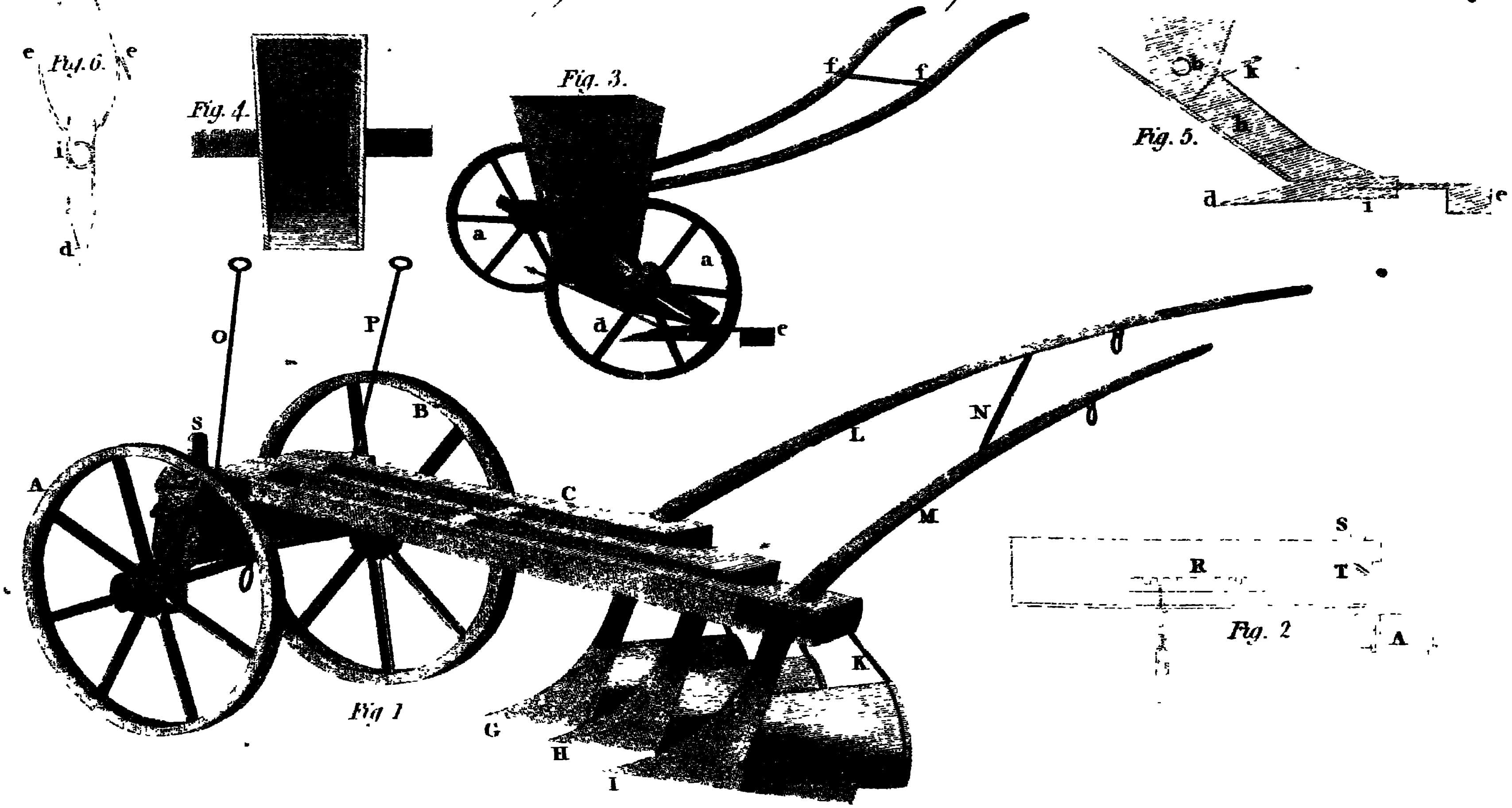


Fig. 3.

Scale of Feet.
7
14

Mason sc. [illegible]

Mr Robert Grews Hand Drill for Peas &c.



The Revd E. Cartwrights three furrow Plough.

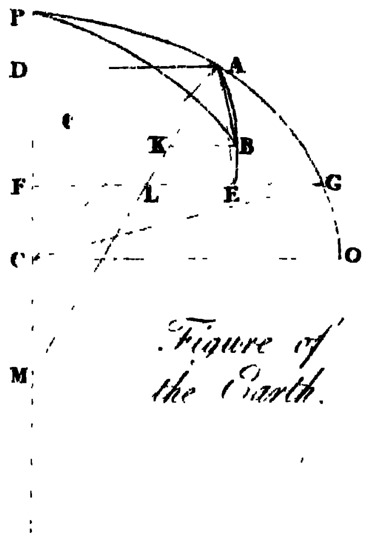


Fig. 1.

Figure of the Earth.

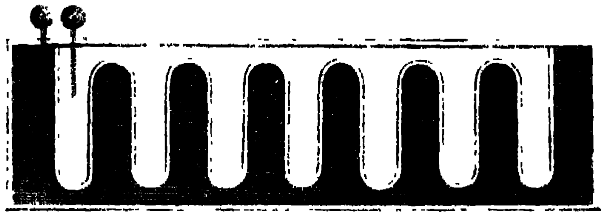


Fig. 2.

Galvanic Apparatus.

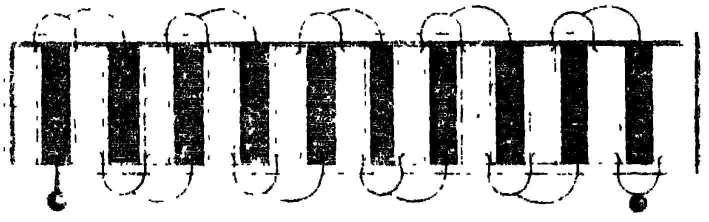


Fig. 3.

New Galvanic Trough. W. N.

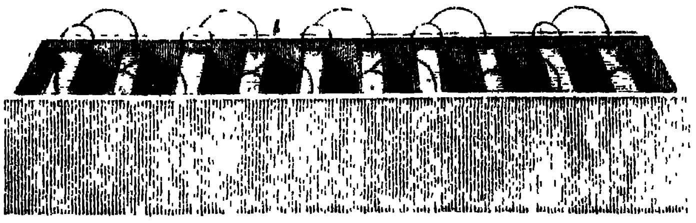


Fig. 4.

Perspective view of Fig. 3.

Drawn by Blunt

Engraved by Mutlow

Hydraulic Machine.

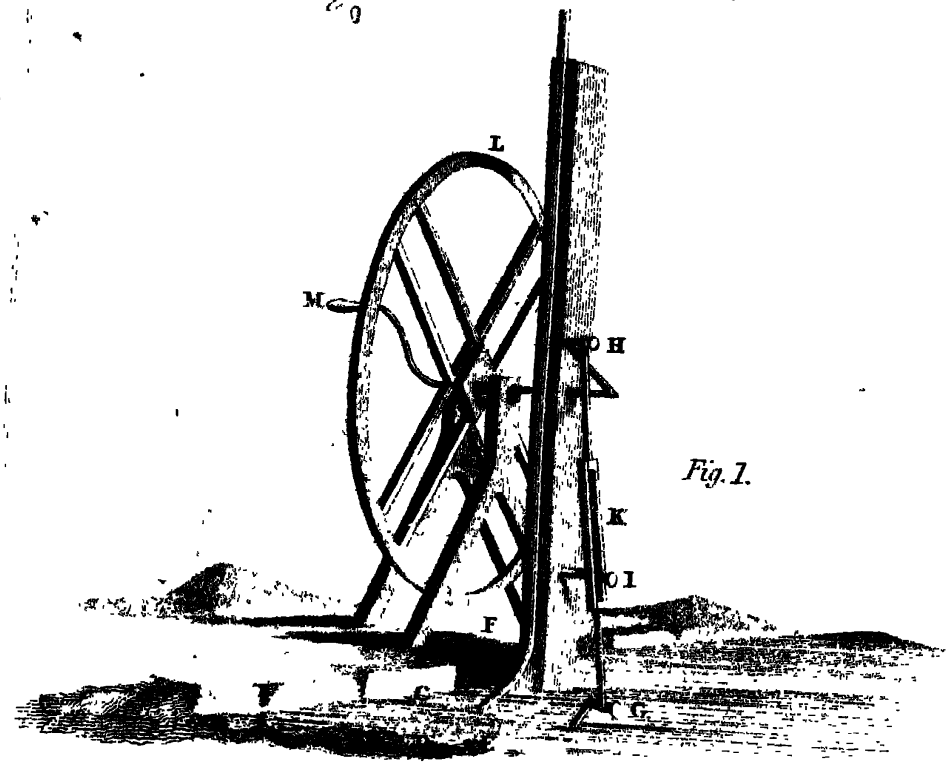


Fig. 1.

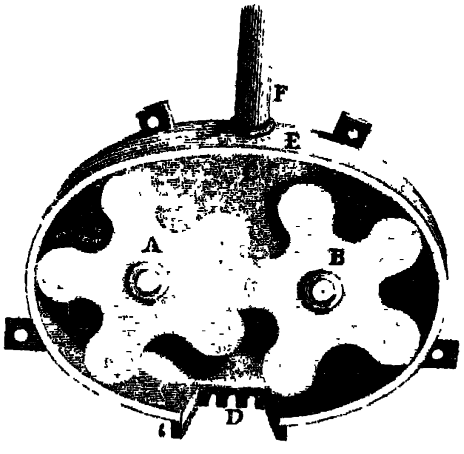


Fig. 2.

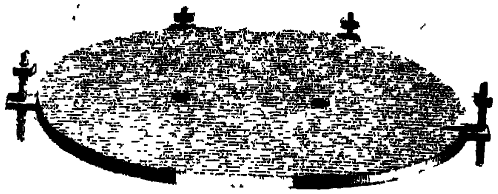


Fig. 3.

Drawn by Hunt

Engraved by Milton

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1804.

ARTICLE I.

Letter from Mr. Ezekiel Walker, *on the Methods of obſerving the Longitude at Sea; with an Exhibition of the very great Accuracy of the mean Reſult from a Number of Chronometers.*

To Mr. NICHOLSON.

SIR,

THE two moſt approved methods of finding the longitude at ſea, are thoſe by lunar obſervations and by time-keepers. Although thoſe methods are now brought to conſiderable perfection, yet they can only be looked upon as in a progreſſive ſtate towards that point of preciſion, at which the aſtronomer and the artiſt have long been labouring to arrive.

Longitude found by lunar obſervations and by time-pieces.

The lunar theory is ſtill but imperfectly underſtood; but perhaps the time is at no great diſtance, when it ſhall be as well known as the theory of any other planet. Nor is it unphiloſophical to ſuppoſe, that ſome property of matter, which is at preſent unknown, may hereafter be diſcovered, by means of which a time-keeper may be ſo conſtructed as to perform as well at ſea as a regulator on ſhore.

Both methods ſtill improveable.

Theſe are conjectures, though probable, that I ſhall not inſiſt upon any further, but proceed to inquire into the merits of the two methods in their preſent ſtate of perfection.

The lunar method will be for ever ſubject to thoſe intervals wherein the moon cannot be ſeen.

The imperfection of the lunar method is, that in conſequence of bad weather, or the moon's not being above the horizon, it ſometimes happens that the neceſſary obſervations cannot be taken for many weeks. And theſe long intervals between one lunar obſervation and another, muſt for ever happen from the ſame cauſes; and thoſe cauſes are ſuch as no human art can remove.

Inſtances of the much greater frequency of obſervations by time-pieces.

La Perouſe, in his voyage round the world, was at one time 31 days without taking a ſingle lunar obſervation; but he had, within that time, 20 obſervations by his time-keeper. At another time during the ſame voyage, 24 days elapſed without a lunar obſervation; but he had 20 obſervations by his time-keeper. Lord Hugh Seymour, in his voyage to the Weſt Indies in the year 1796, had been at ſea near ſix weeks before the firſt lunar obſervation was obtained, in which period the longitude had been obtained 30 times by his time-keepers.

Time-keepers may ſtop; or may alter; or be neglected in winding up; or may ſuffer from accident.

What has juſt been mentioned ſets time-keepers in a very favourable light, but it remains to inquire into their imperfections. 1. A time-keeper may ſtop. 2. It may alter in its rate of going. 3. It may not be wound up through forgetfulneſs. And, 4. It is liable to other accidents, even in the moſt careful hands. But theſe are defects which may be removed, and a reſult obtained, which will be more perfect than by any time-keeper that has yet been made.

Method of obtaining a more perfect reſult from time-keepers; propoſed upwards of 20 years ago.

The method which I have to mention muſt be known to many gentlemen, as I had the honour to lay it before the commiſſioners of longitude in the year 1783. The method, however, ſeems either very little known, or not much attended to. La Perouſe made no uſe of it in his voyage round the world; hence it may be ſuppoſed that it was unknown in France at that time. Nor is it once mentioned by Mr. Wales in his excellent treatiſe on the method of finding the longitude at ſea by time-keepers; although he had "been pretty intimate with the ſubject for near forty years"*.

It conſiſts in taking the mean reſult of a number of machines.

The method which I have recommended depends upon a particular uſe of time-keepers as they are now conſtructed. Although the beſt time-keeper be too imperfect a machine to be depended upon for determining the longitude in long voyages, yet if five, ſix, or more, be taken to ſea in the ſame ſhip, the longitude computed by each ſeparately, and the mean of their reſults taken, it will come exceedingly near the truth, even at the end of three or four months.

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* Preface to Wales on the longitude, p. 5.

This method ſaves the time in caſe of neglect in winding up,

Another very great advantage reſulting from this method is, that, ſhould one of the time-keepers ſtop, it may be ſet going again by the reſt; or ſhould one of them be found to go very incorrectly, its rate may be rejected, and the longitude ſtill determined by thoſe which ſhall be ſuppoſed to go well; in ſhort, this method is almoſt intirely free from all thoſe accidents to which a ſingle time-keeper is liable.

and affords a wonderful degree of preciſion. Examples.

The two following examples, which are taken from the rates of chronometers that have been determined in fixed obſervatories, will ſhow the perfection of this method more clearly. In the firſt example, which contains a period of 30 days, there are only two inſtances in which this method differs ſo much as a ſingle ſecond from mean time. In the ſecond example, containing a period of ſix weeks, the greateſt error on any one day, during that time, is only 5″.9, and the total error at the end of that period, is only 1″.9.

If the chronometers had performed ten times worſe, the error would have been leſs than one minute per month.

It may be ſaid that theſe are favourable ſpecimens, but ſuppoſing that theſe chronometers had gone *ten times worſe* than they did, the greateſt error in the firſt example would have amounted to no more than 11″.3; and the greateſt error in the ſecond example would not have exceeded a ſingle minute.

FIRST EXAMPLE.

Table of the going of three chronometers, and their mean result.

Day of the Month.	*Mr. Arnold's Chronometers.*			Mean Daily Rate of Three Chronometers.	Total Error of Three Chronometers.
	Daily Rate of No. 36.	Daily Rate of No, 51.	Daily Rate of No. 59.		
	Feb. 1779	Feb. 1782	Feb. 1783		
1	+ 0″.79	− 0″.3	− 1″.5	− 0″.33	− 0″.33
2	− 0 .15	+ 1 .1	1 .5	0 .18	0 .51
3	+ 0 .15	− 0 .4	+ 0 .3	+ 0 .02	0 .49
4	0 .00	+ 0 .5	0 .7	0 .40	0 .09
5	− 1 .16	0 .0	0 .0	− 0 .39	0 .48
6	+ 1 .34	− 0 .7	− 1 .6	0 .32	0 .80
7	0 .81	1 .3	0 .4	0 .30	1 .10
8	− 0 .26	0 .0	+ 1 .4	+ 0 .38	0 .72
9	+ 0 .56	0 .5	0 .2	0 .09	0 .63
10	0 .07	0 .0	0 .6	0 .22	0 .41
11	− 0 .45	+ 0 .2	0 .0	− 0 .08	0 .49
12	0 .39	0 .3	− 1 .2	0 .43	0 .92
13	0 .25	0 .6	0 .4	0 .02	0 .94
14	0 .35	0 .9	+ 1 .4	+ 0 .65	0 .29
15	1 .48	− 0 .9	1 .2	− 0 .39	0 .68
16	+ 0 .09	0 .2	0 .2	+ 0 .03	0 .65
17	0 .30	0 .5	2 .2	0 .67	+ 0 .02
18	− 0 .58	0 .7	1 .4	0 .04	+ 0 .06
19	0 .77	0 .1	1 .6	0 .24	0 .30
20	0 .90	0 .9	− 0 .6	− 0 .80	− 0 .50
21	0 .00	0 .0	+ 0 .2	+ 0 .07	0 .43
22	1 .55	0 .0	1 .8	0 .08	0 .35
23	0 .48	+ 1 .0	0 .6	0 .37	+ 0 .02
24	0 .76	0 .7	1 .6	0 .51	0 .53
25	0 .29	0 .9	1 .2	0 .60	1 .13
26	0 .68	1 .0	− 1 .2	− 0 .29	0 .84
27	1 .74	1 .1	0 .8	0 .48	0 .36
28	0 .49	0 .1	+ 0 .5	+ 0 .04	0 .40
Mar. 1	1 .86	0 .4	0 .7	− 0 .25	0 .15
2	− 0 .80	− 0 .7	+ 1 .8	+ 0 .10	+ 0 .23

SECOND EXAMPLE.

Table of three other chronometers.

	Mr. Mudge's Chronometers.		*Mr. Arnold's* No. 86.	Mean Daily Rate of Three.	Total Error of Three Chronometers.
	Green	Blue			
	Daily Rate	Daily Rate.	Daily Rate.		
1	− 1″.12	− 0″.27	− 0″.3	− 0″.56	− 0″.56
2	.62	+ .06	0 .1	.22	0 .78
3	.97	− .40	+ 0 .1	.42	1 .20
4	.61	.40	0 .3	.24	1 .44
5	.16	.16	0 .1	.07	1 .51
6	.11	.05	——	.05	1 .56
7	.64	.41	− 0 .8	.62	2 .18
8	.47	.57	0 .7	.58	2 .76
9	.35	.13	1 .1	.53	3 .29
10	.99	.45	0 .7	.71	4 .00
11	.05	.30	0 .1	.15	4 .15
12	.74	.37	+ 0 .1	.33	4 .48
13	.36	.12	0 .7	+ .07	4 .11
14	+ .04	+ .08	0 .2	.11	4 .30
15	+ .05	− .38	0 .5	.06	4 .24
16	− .26	.34	− 0 .6	− .40	4 .64
17	.00	.45	1 .2	.55	5 .19
18	+ .43	.18	0 .8	.18	5 .37
19	− .36	.49	0 .6	.48	5 .85
20	+ .07	.19	0 .0	.04	5 .89
21	+ .39	.23	0 .0	+ .05	5 .84
22	.14	.17	+ 0 .2	.16	5 .68
23	.59	.04	+ 0 .0	.18	5 .50
24	.49	.46	− 0 .2	− .06	5 .56
25	.65	.46	0 .3	.04	5 .60
26	.20	.71	+ 0 .1	.14	5 .74
27	.86	+ .03	0 .3	+ .41	5 .33
28	.53	− .41	1 .5	.54	4 .79
29	.74	.85	1 .3	.40	4 .39
30	+ .67	.07	0 .2	.27	4 .12
31	1 .50	+ .85	0 .6	.98	3 .14
1	.80	− .03	1 .8	.86	2 .28
2	.87	.23	1 .0	.55	1 .73
3	.63	+ .02	2 .6	1 .08	0 .65
4	1 .28	.16	0 .4	.61	0 .04
5	.43	− .51	2 .2	.71	+ 0 .67
6	.84	.42	0 .8	.41	1 .08
7	.61	.18	0 .6	.34	1 .42
8	.25	.68	0 .2	− .08	1 .34
9	.38	.54	− 0 .6	.25	1 .09
10	.41	.39	0 .2	.06	1 .03
11	− .83	− .27	+ 2 .2	+ .92	+ 1 .95

From

Hence the longitude at ſea may be well determined,

From theſe examples we may ſee the nature of fortuitous events, how regularity riſes out of irregularity; the error of one time-keeper correcting that of another in a wonderful manner; and as the number of time-keepers increaſes, the error will decreaſe, until it be almoſt annihilated. Conſequently the method of finding the longitude at ſea is no longer doubtful, as it may, by this method, be found to any degree of preciſion that may be uſeful in navigation.

and the expence is inconſiderable.

The only objection that can be advanced againſt it is the expence; but the ſum of two or three hundred pounds bears no proportion to the value of a Britiſh ſquadron, nor even to the value of a ſingle Eaſt India ſhip; and this ſum would purchaſe as many time-keepers, at their preſent reduced prices, as would, I preſume, be ſufficient to ſecure any ſhip from that danger which might ariſe from the want of knowing the longitude.

EZEKIEL WALKER.

Lynn, April 19, 1804.

II.

On Galvaniſm. *By* C. Wilkinson, *Eſq.*

May 6, 1804.

To Mr. NICHOLSON.

SIR,

Introductory letter.

BEING now engaged in ſome philoſophical lectures at Bath, I have had frequent opportunities of converſing with Dr. Gibbes, a gentleman of conſiderable ſcientific information, relative to his opinions as to the apparent decompoſition of water by the galvanic proceſs. As many gentlemen of eminence in this department of philoſophy are converts to his doctrine, I beg leave, through the medium of your valuable Journal, to ſtate the outlines of Dr. Gibbes's theory to the public, with a few curſory obſervations which have occurred to me.

I am, Sir,
Your's with great reſpect,
C. WILKINSON.

No. 19, *Soho-Square.*

In the decompoſition of water by galvaniſm the effects are produced at great diſtances aſunder.

Soon after the publication of the valuable diſcovery of the decompoſition of water by the galvanic proceſs, by Meſſrs. Nicholſon and Carliſle, Richter, an eminent German philoſopher, obſerving that water becomes decompoſed by wires placed at a conſiderable diſtance from each other, could not conceive that the ſame identical particle of water could be influenced by two wires at the ſame inſtant of time; and hence he conjectured that the Lavoiſierean theory relative to water being a compound ſubſtance muſt be incorrect.

Dr. Gibbes ſuppoſes the oxigen to be water and plus electricity, and the hidrogen to be water and minus electricity.

Dr. Gibbes ſuppoſes water to be an elementary principle, and that the two gaſes which are produced are compounds. Thus the oxygen gas which is diſengaged from the zinc end of the battery, is taken to be a compound of water and poſitive electricity; the water conſtituting its ponderable part, while the hydrogen gas given out by the other wire connected with the copper end of the battery is concluded to have water alſo for its ponderable part in a ſtate of combination with negative electricity.

That plus and minus electricity compoſe caloric.

Dr. G. conſiders poſitive and negative electricity as two diſtinct principles, which when in a ſtate of union conſtitute caloric. Thus in the exploſion of a mixture of oxygen and hydrogen gaſes, the two electricities enter into union and form caloric, while the reſpective ponderable baſes are precipitated in the ſtate of water.

Hence flame and heat,

Upon this hypotheſis, the circumſtances of the evolution of flame and heat, together with the production of water, are capable of explanation.

and the galvanic phenomena.

Alſo upon this ſuppoſition ſome notion may be formed, why the gaſes are produced in the vicinities of each wire, and why oxygen gas muſt be evolved from the wire connected with the zinc end of the battery, and hydrogen gas from the wire united with the copper end.

Examination of the theory.

This ingenious ſuppoſition, which does great credit to the well-known abilities of Dr. Gibbes, in the firſt point of view ſeems to receive additional ſupport from its apparent ſimplicity. But upon more minute inveſtigation, I am induced to think it will be found more complicated than the Lavoiſierean theory, and by no means ſo general in its application.

The two electric principles not proved.

In the firſt place it ſhould be proved that there are two ſuch diſtinct principles as poſitive and negative electricity; and in ſupport of this, the experiments of Eeles, Symner, Atwood, &c. are by no means concluſive,

When

Nor the decompoſition of ammonia explained.

When pure ammonia is expoſed to the galvanic action, we have, as before, hydrogen from one wire, and nitrogen from the other. How can theſe productions be explained upon the principle of Dr. Gibbes?

This theory ſuppoſes oxidation to be the addition of water and ſubtraction of electricity. But the pile requires oxigen *from the atmoſphere.*

This able phyſician further obſerves that oxydation is a combination of water with the metallic body, conſonant to the opinions of Prieſtley, Watt, and others.

When a galvanic pile is placed under a receiver ſituated over water, while the galvanic proceſs goes on, by having a metallic circuit from the top to the bottom plate, a loſs of air enſues, and which is found to be the pure part of atmoſpheric air: if the pile be placed in an exhauſted receiver, the galvanic proceſs is very trivial, and the plates very ſlightly oxydated. How upon the principles of Dr. Gibbes can we explain theſe phenomena? If oxydation be merely the combination of water with the metal, why ſhould the pure part of atmoſpheric air be thus ſeparated while the metals are in immediate contact with ſo much water?

Oxidation appears to require the diſengagement of electricity.

Dr. Aſh's experiment.

Whether galvaniſm be the cauſe or the effect of the chemical change induced in the metallic ſubſtance, they appear to me to be contemporaneous reſults, ſimilar to what appears in the immerſion of iron in a ſolution of the ſulphate of copper, and whether the iron be diſſolved prior to the copious precipitation, no experiments I am aware of can decide. From various circumſtances it appears that oxydation cannot take place unleſs the combined electricity of the metal be capable of being diſengaged. This is rendered evident in a very happy experiment of Dr. Aſh, who having been the aſſociate of the much-lamented Humboldt* in his ſcientific purſuits, may now be deemed more acquainted with the minutiæ of galvaniſm than any other perſon. This gentleman has remarked that when a plate of zinc is immerſed in a weak ſolution of ſulphuric acid and water, a decompoſition takes place, the oxygen baſe combines with the metal, while the hydrogen is diſengaged in a gaſeous ſtate. When a plate of ſilver is immerſed, no decompoſition takes place; the very inſtant a contact is effected between the two metals, whether by portions out of the fluid or in the fluid, then the ſilver is immediately acted on and diſengages hydrogen alſo, and itſelf becomes oxydated.

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* Humboldt died lately at Acapulco, of the yellow fever, while attempting to perfect his geological obſervations.

Explanation.

In the firſt inſtance the evolution of electricity appears to be the reſult of a chemical action of the ſulphuric mixture on the zinc, while in the latter, no action is induced on the ſilver till ſome alteration is effected as to its electrical ſtate; as we well know from other galvanic experiments, that of zinc and ſilver, when thus combined, the zinc evinces poſitive ſigns, and ſilver negative; it is hence evident that an abſtraction of electricity from ſilver is requiſite prior to its capability of being acted on by the ſulphuric acid.

Additional remarks.

If I might preſume to ſubmit to ſome of your chemical readers, an examination of ſuch compound of ſilver as may thus be effected; a ſulphate of ſilver is actually formed, it might lead to ſome important diſcoveries. I am every day more and more perſuaded that light and electricity are the two moſt active agents we poſſeſs, and a more minute enquiry into their reſpective energies, I have no doubt, would enable us to explain many phyſical changes in the material world, with which we are at preſent perfectly unacquainted.

Intenſe light from galvanic combuſtion.

The very intenſe light diſengaged from charcoal by means of my extenſive apparatus, has occaſioned me to be frequently honoured with the attendance of Dr. Herſchell; from ſome converſation I have had with him, I am in hopes he will be induced to inſtitute ſome experiments relative to the evolution of light, ſo nearly approaching to ſolar light. If theſe ſhould be conducted, I ſhall do myſelf the honour of ſtating them to the public through the medium of your valuable Journal.

III.

On the Formation of Snow. By G. A.

To Mr. NICHOLSON.

SIR,

THE remarks contained in the encloſed pages, may not, perhaps, evince any originality of thought, nor more than would occur to the moſt ſuperficial obſerver; yet the ſuggeſtions of ſome perſons, frequently lead to the obſervation of valuable facts by others. Therefore, Sir, you will make whatever application of the paper you conſider beſt.

If you think it worthy of inſertion in your valuable Journal, it will be conſidered as an honorary obligation conferred on,

Your's, moſt reſpectfully,

23d April, 1804. G. A.

On the formation of ſnow;—

The frequent changes of the weather that have taken place during the laſt winter, having induced me to direct my attention to meteorology; I confeſs, that the manner in which philoſophers account for ſome of the phenomena that occur, is not, to me, altogether ſatisfactory.

—ſuppoſed to be effected by electricity.

It is not ſurprizing, that electricity (with the immediate agency of which we are ſo little acquainted) ſhould be reſorted to, as the grand agent in all meteorological phenomena. Accordingly we find, that ſnow, and indeed every variety of weather we experience, is conſidered to be more or leſs effected by the electric fluid.

Snow is generally ſuppoſed to be the vapours of the atmoſphere, diſengaged by the electric fluid, and frozen.

Queſtions reſpecting the mode, &c.

But it appears to me, that before we receive ſo vague an explanation, the following queſtions might be aſked:—

What are the vapours of the atmoſphere compoſed of?

By what laws, and in what manner does the electric fluid act, either in the formation of ſnow, or as a component part of it?

Electricity ſuppoſed not eſſential to ſnow;

I ſhall now offer a few remarks to ſtrengthen a ſuppoſition, that the electric fluid is not engaged in, or in the leaſt eſſential to the production or exiſtence of ſnow.

—but a change of wind.

By an attentive obſervation of all the circumſtances that have attended the fall of ſnow, during the laſt winter, I have, in almoſt every inſtance, found that it is accompanied with, or rather preceded by a change of the wind; and that the wind, previous to the fall of ſnow, blew from ſome point between the *South*, and the *Weſt*; and afterward from ſome point between the *Eaſt*, and the *North-Weſt*.*

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* If it is obſerved, that we ſometimes have ſnow, without the wind changing to any of the points above-mentioned, or, even *without a viſible change* to us; yet it does not militate againſt the following remarks; for it has been obſerved by aeronauts, that different ſtrata of air blow from *oppoſite points* at the ſame time.—Therefore, notwithſtanding a ſouth wind may prevail at the ſurface of the earth, a ſuperior ſtratum may blow from the North.

Such being the facts, is it not probable, that *a change of the wind is the cauſe of ſnow?*

Now let us examine, whether ſuch a cauſe will produce ſuch an effect.

The winds that blow from any of the points between the *South* and the *Weſt*, by coming from warm climates, and paſſing over, perhaps, a very large tract of water, where there is a powerful evaporation going on, muſt poſſeſs a very great degree of humidity, and are moſt commonly, of a temperature between 45° and 60° of Fahrenheit.

Warm winds meeting cold depoſit their vapours in ſnow.

The winds which blow from any of the points between the *Eaſt* and the *North-Weſt*, by coming moſtly from ſuch high latitudes, and paſſing over immenſe fields of ice, where evaporation is undoubtedly greatly impeded, cannot be ſuppoſed to contain much water in ſolution, but muſt bring with them very great degrees of cold.

Namely, north-eaſterly meeting ſouth-weſterly winds.

Now let us ſuppoſe that a north wind of any temperature between 32° and 0° (which it generally is, in ſuperior ſtrata of the atmoſphere) meets a ſouth-weſt wind, as before-mentioned, the conſequence will be, that the intenſe cold which accompanies the former will convert the water with which the latter is impregnated into ice; and the inſtantaneous application of cold is probably the reaſon why ſnow is produced in what we call *flakes;* for before the vapour can concentrate itſelf into large particles, or drops, it is arreſted by the intenſe cold.

In this view, the formation of ſnow appears to be a beautiful chemical phenomenon; for the warmer air, having a greater affinity for the colder air than it has for the water which is held in ſolution, the water is diſengaged, cryſtallized by the cold, and precipitated in the form of ſnow.

Formation of ſnow.

It is generally obſerved, that it is unuſually cold for half an hour or an hour before the fall of ſnow, and warmer afterwards. Might not this be accounted for, by conſidering that the adverſe wind muſt meet with conſiderable reſiſtance, in effecting either a *union* with, or a *paſſage through* a ſtratum of air ſurcharged with water, and conſequently muſt be in a great degree reflected back again, not in the perpendicular, but as radii from a center, in an oblique direction, part of which muſt deſcend to the earth. And it will undoubtedly be warmer, after

Formation of ſnow.

after the ſtratum of north wind has either forced a paſſage through or effected a union with the ſouth-weſt wind.*

Though I have not, in the preceding obſervations, conſidered the electric fluid as at all eſſential to the production of ſnow, yet I do not deny the preſence of it. That ſnow contains the electric fluid, cannot be doubted; but it does not follow, that the latter is *neceſſary* to the exiſtence of the former. We know of no ſubſtance in nature, that is impervious to that ſubtile fluid; it ſeems to pervade *all bodies* with nearly the ſame facility as caloric. Therefore, though ſnow indicates electricity, it is probably no more than it has acquired in its paſſage through an electrified atmoſphere.

If thoſe who are much more competent to the taſk than myſelf, would direct their attention to this moſt intereſting branch of natural philoſophy, I am inclined to think, they would find the reſult of their enquiries highly gratifying. Meteorology cannot be conſidered, but as yet in a ſtate of infancy; for the greater part, our knowledge of it is hypotheſes, which we cannot ſupport by experiment. Therefore, it is only by a cloſe obſervation of facts, accompanied by juſt inferences drawn from them, that we can arrive at any degree of certainty on this complicated ſubject.

IV.

Medico Chemical Reſearches on the Virtues and Principles of Cantharides. By H. Beaupoil.†

Experiments and obſervations on cantharides.

THOUGH the animal kingdom preſents us with only a ſmall number of ſubſtances of uſe in medicine, it muſt nevertheleſs be admitted, that among thoſe we poſſeſs, there are ſome of which the effect is ſo certain, ſo conſtant, and ſo definite, that if we were deprived of them; it would be difficult, and perhaps impoſſible, to find others to ſupply their place.

Cantharides are of this number; their mode of action is univerſally known, together with the advantages they afford in many diſorders. It is not, therefore, to be wondered that the

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* The water gives out heat in congelation. *Vide* Irwine, Black, Crawford, &c.

† *Annales de Chimie*, XLVIII. 29.

Experiments and obſervations on cantharides.

examination of theſe inſects ſhould have engaged the attention of celebrated phyſicians, and that their analyſis ſhould have been frequently attempted by chymiſts.

The principal aim of all thoſe who have operated upon this material, has been to diſcover whether the bliſtering property which it ſo eminently poſſeſſes does generally appertain to all the parts of the animal, or whether it do not rather reſide in ſome peculiar matter, which, independent of the parts which accompany it, is capable of acting alone, and producing the effects which are obſervable by the entire cantharides.

It is undoubtedly needleſs to repeat in this place, what has been ſaid and done reſpecting this object; but it is eſſential to remark, that no one before Thouvenel purſued that courſe which could lead to the ſolution of the problem offered for conſideration; and accordingly, we muſt conſider the period in which that phyſician publiſhed his experiments on cantharides, as the earlieſt time in which philoſophers could indulge the hope of aſcertaining ſome poſitive information reſpecting the nature and properties of the immediate material of thoſe inſects.

But while we tender juſtice to the labours of Thouvenel, we muſt confeſs that he has not carried them to an extent anſwerable to his happy commencement. For he has neglected ſome of the moſt important queſtions, and among others, thoſe which relate to the veſicatory, diuretic, and aphrodiſiac properties of cantharides.

It was to ſupply in ſome meaſure the deficiency of that reſpectable philoſopher upon the above three points, that Citizen Beaupoil has thought fit to undertake a new examination of cantharides. The paper in the annals conſiſts of an extract from his memoir, by Citizen Deyeux.

The author divides his diſſertation into four parts.

In the firſt he gives a rapid ſketch of the ſpecific properties of cantharides; the methods uſed for collecting them, and the preparations to which they are ſubjected previous to their introduction to the market as an article of commerce.

In the ſecond he gives a ſlight hiſtory of the uſe and application of theſe inſects from the time of Hippocrates to the preſent period.

In the third we find an accurate outline of the attempts made by chymiſts to analize the cantharides, as well as an account of his own particular experiments and their reſults.

The

Experiments and obſervations on cantharides.

The fourth contains every thing which relates to the phyſiological eſſays made with theſe animals; the effects produced by their exhibition, whether internally or externally; and laſtly, obſervations relative to the opening of the bodies of ſeveral dogs, to which the author had given either the entire cantharides, or the different immediate materials, which he ſeparated by means of his proceſſes.

As the firſt and ſecond parts contain nothing more than is to be met with in various authors, it is unneceſſary to attend to them, and accordingly, the abridger has confined himſelf to the third part, in which the chemical facts are given.

Thouvenel, who was the firſt rational experimenter on cantharides, made uſe of water and alcohol to ſeparate the ſoluble parts of theſe inſects, and the reſults he obtained were,

1. A yellow reddiſh extractive matter, of a ſharp bitter taſte, reſembling, as he ſays, that of ants, but leſs acid.

2. Another yellow matter, of a paler colour than the former, and nearly inſipid.

3. A fatty matter, of a green colour and acrid taſte, poſſeſſing the ſmell by which the entire cantharides are diſtinguiſhed.

4. Laſtly, A parenchymatous matter.

Citizen Beaupoil obtained ſimilar products; but he not only aſcertained their exiſtence, but examined them ſeparately, and in this it is that the difference between his labours, and thoſe of Thouvenel principally conſiſts.

He firſt obſerved that the aqueous ſolution of the peculiar extractive matter afforded by cantharides, does not fail to undergo a kind of alteration when expoſed to the air; that the fluid becomes turbid, affords a yellowiſh precipitate, which acquires a peculiar odour; becomes covered with a viſcid pellicle, emitting a fœtid ammoniacal ſmell; and that when it has arrived at this term, the fluid no longer exhibits any ſenſible change. He afterwards remarked that the ſolution here mentioned before it undergoes thoſe changes which are produced by expoſure to the air, ſtrongly reddens the tincture of turnſole; that when mixed with rectified alcohol or ether, it becomes divided into two parts, nearly equal; the one poſſeſſing the form of a black adheſive precipitate, inſoluble in alcohol, and the other that of a yellow brown matter, very ſoluble in that fluid.

(The Concluſion in our next.)

Deſcription

V.

Deſcription of a Galvanic Apparatus affording a large Surface for Oxidation, and convertible into one or more Plates at pleaſure. By I. R. I.

To Mr. NICHOLSON.

SIR, *Edinburgh, May* 10, 1804.

I AM much flattered by the notice that you and Mr. Wilkinſon have been pleaſed to take of my late communication, and I think I have done no ſmall ſervice to ſcience by drawing forth from both of you, the very ingenious diſquiſitions towards the œconomy of galvaniſm, which appeared in your laſt Journal.

Improvement in the galvanic trough.

Your deſcription of what may be called a Polychreſt trough, ſeems to reach as near perfection, in every requiſite property for giving the ſhock, or deflagrating metals, as could be wiſhed: Yet there is ſtill a further improvement which has ſuggeſted itſelf to my mind; but as it is probable the waſte of the zinc would be more conſiderable, I mention it with diffidence.

Corroſion greateſt in the *couronne de taſſes.*

It appeared to me in ſome experiments I made with the *couronne de taſſes,* that the metals were more corroded to produce the ſame effect, than happened either in the pile or the trough; but if Mr. W.'s idea is juſt (which is moſt likely to be, from his great experience in the ſcience), that the effect is in proportion to the oxigenation of the metal, the following plan appears the beſt adapted for both œconomy and power.

Deſcription.

In Fig. 3, Plate VI. A repreſents, by a ſide view or ſection, a plate of zinc of ſix inches by three; in its centre is a ſquare piece, from which riſes another piece of a ſmaller diameter, either ſquare or round. Theſe projecting bits are both ſunk in a block of wood ſomewhat larger than the whole plate, but not ſo as to paſs through it; and the plate muſt ſtand clear of the wood and every part of the cell, hanging entirely by the knob, and ſtrongly cemented. A hore is made down through the thickneſs of the wood, in a line with one through the ſmall knob of the zinc.

The copper wire B, paſſed through that hole, is the medium of communication from cell to cell; and the ſame mode may be employed in forming them into one plate, as ſuggeſted, in your laſt, when a deflagrating power is required.

The intermediate parts expreſs the wooden blocks, which muſt be well covered with cement on every ſide, and inſerted into grooves, as the zinc and copper plates uſually are.

The engraving that referred to my laſt communication, was inaccurately explained, as the dotted work was ſaid to expreſs the zinc, whereas the acid liquor was denoted by it.

I remain, Sir,
With much reſpect,
Your obedient ſervant,
I. R. I.

VI.

On the Difference between the Effects of Electricity and of Heat.

By *Cit.* BERTHOLLET[*].

Difference between electricity and caloric.

I HAVE thought it important to determine the difference which exiſts between the action of the electric fluid and that of caloric, and the cauſe which renders their effects ſimilar, more eſpecially, as in the leſſons of the Normal ſchools this ſimilitude of effect made me adopt the opinion of thoſe who have conſidered the electric fluid to be caloric itſelf; I conſequently requeſted permiſſion of Citizen Charles to make uſe of his powerful apparatus in the experiments which appeared to me to be neceſſary on this ſubject. With that civility which thoſe engaged in ſimilar purſuits are always ſure to experience from him, he undertook to perform them himſelf. I now give the reſult, ſuch as it was communicated to me by Guy Luſſac, who aſſiſted in the experiments.

Platina was not much heated by an electric ſhock nearly capable of burning it.

A wire of platina was ſubmitted to ſhocks which were nearly ſtrong enough to effect its combuſtion; and to be ſatisfied of this, a ſhock was excited by which a great part of the wire was melted and diſperſed; afterwards the ſhocks employed were a little weaker, and immediately after each the wire was touched to judge of the temperature it had acquired: a heat was felt, which was diſſipated in a few minutes, and which, at the utmoſt, was eſtimated to reſemble that of the boiling point of water. If electricity liquefied metals and brought them into combuſtion by the heat it excites, the platina wire muſt, after a ſhock which differed but little from that which would have produced its diſperſion and its combuſtion, have approached the degree of temperature which occaſions its liquefaction; now this degree, which is the moſt elevated that can be obtained, would, according to the valuation, more or leſs accurate, of Wedgwood, be 32277° of Fahrenheit.

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[*] From his Eſſai de Statique Chimique.

When the ſhock is ſufficiently ſtrong to deſtroy the aggregation of the platina wire, it begins by detaching moleculæ from its ſurface, which exhale like ſmoke; if it is ſtrong enough to produce combuſtion, the remains of the wire appear to be torn into filaments.

Electricity detaches the parts of platina, but does not fuſe it.

A thermoſcope, blackened with ink, and placed in the ſtream of a ſtrong electric ſpark, only experienced a dilatation which was nearly equal to one degree of Reaumur's thermometer; and this ſlight effect might depend on the oxidation of the iron of the ink: placed beſide the current, it did not ſhew any dilatation, although the air was neceſſarily affected by the electric action: it was the ſame when it was placed in contact with a metallic conductor which received a ſtream leſs powerful than in the preceding experiments.*

The electric current of ſparks cauſes little heat.

A cylinder of glaſs filled with air, with an exciter at each of its extremities, to one of which was fixed a tube, communicating with another cylinder filled with water, produced an impulſe at each ſhock which raiſed the water more than a decimeter above its level; but its effect was inſtantaneous.

Electric ſhock through air.

Theſe experiments ſeem to me to prove that electricity does not act on ſubſtances, and on their combinations, by an elevation of temperature, but by a dilatation which ſeparates the moleculæ of bodies. The ſlight heat obſerved in the platina wire is only the effect of the compreſſion produced by the moleculæ which firſt experience the electric action, or which experience it in a greater degree; it muſt therefore be compared to that excited by percuſſion or compreſſion.

Hence it is inferred, that electricity acts not by heating, but dilating.

If the dilatation was the effect of heat, that experienced by a gas in the experiment related above, would not have been inſtantaneous, it would only have experienced a progreſſive diminution by cooling, as when its expanſion is owing to heat.

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* A ſmall thermometer in the luminous current between two balls of wood, is raiſed 32 degrees.—Nairne.

When ammoniacal gas is decompoſed by electricity, it remains cold.

In the experiment by which ammoniacal gas is decompoſed, the gas indubitably receives the electric action, and nevertheleſs it is not heated; and as ſoon as the decompoſition is finiſhed, its volume remains unchanged, becauſe the electric action which is employed in this experiment, is not ſufficiently energetic to cauſe a perceptible dilatation. No ſenſible dilatation is produced in a gas by a ſhock which is not very ſtrong, becauſe the impulſe not being gradual, like the expanſion ariſing from caloric, and being excited inſtantaneouſly, the reſiſtance of the liquid becomes too great, and cannot be overcome unleſs the dilatation has great energy.

Lead exploded in azote continues metallic, and is not fuſed.

An experiment of Dieman and his learned aſſociates c[illegible]ms this explanation: They cauſed a ſhock to paſs [illegible]d placed in a veſſel filled with azote gas, which could [illegible] oxidate it; it was reduced into powder retaining al[illegible] metallic properties: If it had experienced a liquefaction ſimilar to the action of heat, it would have cooled gradually, and would have congealed into one, or at leaſt into ſeveral maſſes.

No heat is produced by the electric dilatation of metals: the ignition and heat are from oxidation or combuſtion.

When a metal is ſubmitted to the electric action, the effects produced immediately by the electricity muſt be diſtinguiſhed from thoſe which are owing to its oxidation: The firſt are limited to the diminution or deſtruction of the effects of the force of coheſion, to removing and diſperſing the moleculæ; (if by this a little heat is diſengaged, it is only owing to the compreſſion ſuſtained by ſome of the parts); but thoſe which are occaſioned by the oxidation, produce a high degree of heat, and then the effects aſſume all the appearances of an ordinary combuſtion; hence it ariſes that the moſt oxidable metals are thoſe which become red with the greateſt facility, and which moſt ſhew the properties of a metal liquefied by heat.

Electricity favours oxidation by diminiſhing coheſion.

Electricity favours this oxidation, in as much as it diminiſhes the force of coheſion; it is thus that an alkali renders the action of ſulphur on oxigen much more powerful, by deſtroying the force of coheſion oppoſed to it, and that a metal diſſolved in an amalgam is oxided much more eaſily than when it is in a ſolid ſtate. It is only by deſtroying the effects of the force of coheſion that heat itſelf produces the oxidation of metals, but the expanſive action of electricity will have a great advantage over that of caloric, becauſe its action is confined to the ſolid which it encounters in its courſe, ſo that the gas itſelf will not experience a dilatation in oppoſition to the condenſation which accompanies the combination: To this circumſtance may be applied what is obſerved in the action of hidrogen gas, which is capable of completely reducing an oxide of iron placed in the focus of a burning glaſs, although water, whoſe two elements receive the heat equally, is decompoſed by this metal.

Oxidation of metals by electricity and water.

It is probable that it is alſo to the expanſive effect of an electric current eſtabliſhed between two metals, having a ſtratum of water interpoſed, that the oxidation obſerved by Fabroni between theſe ſubſtances, placed in contact in water, is owing, and which, in this caſe, appears to be confined to the combination of the oxigen which is held in ſolution in this liquid*.

This explanation of the agency of electricity is general, but does not ſhew the difference between plus and minus.

All the chemical effects produced in ſubſtances ſubmitted to the action of electricity, ſeem capable of being deduced from theſe conſiderations, and of being explained by the diminution of the force of coheſion, which is an obſtacle to the combinations which their moleculæ tend to form; but the differences which may be offered by poſitive electricity and negative electricity remain to be determined: the chemical effects of the pile of Volta may be much more conſiderable than thoſe of the common electricity, although the latter poſſeſſes a much greater tenſion, becauſe its action being neceſſarily interrupted, the chemical effects which require time to be accompliſhed, can only begin to be effected, and may even be deſtroyed, by the ſudden re-eſtabliſhment of the firſt ſtate of the body; while the permanence of the action of the electromotive apparatus, although weaker at each inſtant, may give riſe to the chemical changes which it promotes, by diminiſhing the effects of the force of coheſion.

Galvaniſm may owe its greater energy to its conſtancy.

I do not myſelf conſider the explanations I have now hazarded as more than conjectures, which obſervation may confirm or deſtroy.

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* Journ. de Phyſ. Vendem. An. X.

VII.

Letter from a Correſpondent I. R. I. *explaining ſome Facts in Galvaniſm, and on other Objects.*

To Mr. NICHOLSON.

SIR, *Edinburgh, May* 19, 1804.

Obſervations on galvaniſm.

I SENT you, a few days ago, ſome hints towards a farther improvement of the galvanic apparatus*, and I hope that the following obſervations will arrive in time to be added as a ſupplement to my laſt communication.

There ſeem to be two very marked laws in galvaniſm; the firſt is, that the phænomena are produced by moiſt oxidation, which ſeems a general one; but, by the ſecond law, that they remain latent till one part of the conducting metal be made perfectly dry: this is at leaſt ſtrictly correct where two metals are employed.

From the tendency of galvanic effects to become latent, by the uſe of moiſt conductors, a pile cannot be formed unleſs one ſide of the oxidated metal be covered from moiſture by cement, beſides having a conductor of a different metallic ſubſtance. The trough is, therefore, beſt adapted to œconomize galvaniſm, as I have ſhewn; that nearly as large a ſurface can be expoſed to the action of the fluid in it, as in the *couronne de taſſes*, over which it has many advantages of ſteadineſs, portability, &c.

From the neceſſity of dry conductors to alternate with moiſt to produce the more ſtriking and perfect galvanic effects, it would appear that the former give the neceſſary celerity to their action; it might therefore be uſeful to enquire, whether making them of a conſiderable length, and incloſing them in ſome ſubſtance that would inſure them from the moiſture inſeparable from the galvanic apparatus, might not conſiderably increaſe that celerity.

It is much to be wiſhed that the end conductors were improved; jointed wires ſeem to loſe much of the power, by not being in perfect contact; ſpiral wire ſeems better adapted to that uſe, and more capable of varying in its direction.

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* See page 79.

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Obſervations on galvaniſm.

It does not ſeem impoſſible to take a ſhock of its full force from piles or troughs of different powers, by bringing their conductors very near, but keeping them from actual contact by pieces of ivory or baked wood: it does not ſeem ſo clear that the full power of combuſtion could be preſerved in the ſame manner.

I was lately rather ſurprized to find that galvaniſm had the effect of making ſilver remarkably brittle: this looks as if its action was ſomehow connected with the malleability of the perfect metals, and to ſhew that the conducting metals ſhould be occaſionally paſſed through the fire: the ſilver plates were nearly clean.

Attraction of moiſture by oxide of cobalt.

I will conclude with an obſervation on another ſubject. Having kept a conſiderable variety of Reeves's water-colour cakes in a damp cloſet, many of the cakes were in ſome degree affected by it ſo as to acquire mold; but the colour that Reeves ſells under the name of Royal Smalt, has a moſt ſurpriſing power of attracting moiſture, as it was reduced moſt completely to a ſoft maſs; as almoſt all the vegetable, animal, and mineral ſubſtances uſed in painting were in the ſame box, we may pretty fairly conclude that an oxide of cobalt ſurpaſſes them all in its power of attracting moiſture, and would make a moſt delicate hygrometer if ſuſpended from one arm of a fine beam.

I am, Sir,
With much reſpect,
Your obedient ſervant,
I. R. I.

VIII.

On the Preſence of a new earthy Phoſphate, found in the Bones of Animals, which does not exiſt in thoſe of Men. By FOURCROY *and* VAUQUELIN. *Read before the National Inſtitute.*[*]

Recapitulation of the analyſis of urine, &c.
No phoſphate in the urine of animals,

WE flatter ourſelves the Inſtitute will recollect our laborious eſſays on the analyſis of urine, and ſtony concretions found in the body of man and animals; with the ſtriking difference exiſting between them, and the cauſe to which the latter is owing. We endeavoured to prove in thoſe eſſays that none of the ſalts called phoſphates, (which exiſt in abundance in the urine of man) are to be found in the urine of mamiferous animals, that their kidneys are not the concretory of theſe ſaline combinations; but that the hair which covers their ſkin, and the corneous appendages which defend their extremities, are the organs and repoſitories in which nature ſecretes and depoſits theſe ſalts in the body of animals.

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[*] Gehlen's New Journal of Chemiſtry, I. 555.

Nor the morbid concretions called urinary calculi.

We have alſo ſhown that the morbid and preternatural concretions, called urinary calculi of man, contain beſides uric acid, alſo phoſphate of lime and phoſphate of magneſia; none of which are met with in thoſe of animals; and that on the contrary, the ſtony concretions found in the inteſtinal canal of animals, always contain phoſphates of different kinds, whereas the concretions met with in the inteſtines of man, do not contain a veſtige of them.

The bones alſo differ. Thoſe of animals contain phoſphate of magneſia; thoſe of man do not.

We ſhall now endeavour to prove that the bones of animals alſo differ in compoſition from thoſe of men. We have found that the former contain, beſides phoſphate and carbonate of lime, alſo phoſphate of magneſia; the latter of which has hitherto eſcaped the notice of chemiſts. This ſalt which we have detected in the bones of all the animals we hitherto examined, does not at all exiſt in the bones of man. We ſhall firſt ſtate the method we employed for detecting and ſeparating it, and then point out the relative proportions in which it exiſts in different animals.

Method of ſeparating magneſia from the bones of animals.

Take a quantity of bones of adult animals, burn them to whiteneſs in an open fire, and reduce them to a fine powder. Upon one part of this powder, after having been put into a convenient earthen or glaſs veſſel, affuſe an equal quantity by weight of concentrated ſulphuric acid; ſtir the mixture intimately together, and then ſuffer it to ſtand for five or ſix days. Having done this, dilute the maſs with at leaſt ten times its bulk of water, agitate it well and tranſfer it on a ſtrainer. When no more fluid paſſes, diffuſe the maſs again through five times its quantity of water, and ſtrain again as before, and repeat this proceſs till the water runs taſteleſs. The fluids thus obtained, are to be added together, and mingled with liquid ammonia, taking care the latter be in exceſs. The precipitate which is obtained conſiſts of phoſphate of lime, phoſphate of ammonia, and phoſphate of magneſia. To ſeparate

theſe

Method of ſeparating magneſia from the bones of animals.

theſe ſalts, waſh it in as little cold water as poſſible, and boil it in a ſolution of potaſh ſo long, till the odour of ammonia is no further perceptible. The potaſh in this proceſs decompoſes the phoſphates of ammonia and magneſia, leaving the latter and phoſphate of lime. To ſeparate theſe two, let the whole be boiled in acetic acid, the magneſia will be diſſolved, and the phoſphate of lime remain untouched.

To obtain the magneſia, let the ſolution of acetite of magneſia be carefully evaporated to dryneſs, re-diſſolve the ſalt in water, and decompoſe the ſolution by carbonate of ſoda; the precipitate obtained is carbonate of magneſia. If it be pure carbonate of magneſia, it will be completely ſoluble in ſulphuric acid; if it contains lime, the ſolution will be cloudy, and a precipitate will gradually be depoſited.

Such is the method we employed for detecting and ſeparating the magneſia contained in the bones of animals; it is perhaps tedious, but it is eaſy and certain.

The bones of the ox examined in this manner yielded ſomething leſs than $\frac{1}{16}$ of its weight of ſulphate of magneſia, which is equal to about $\frac{1}{14}$ of phoſphate of magneſia, or in the burnt bone to $\frac{1}{24}$.

The bones of the horſe and ſheep afforded $\frac{1}{36}$ of phoſphate of magneſia.

Thoſe of fowls and fiſh yielded nearly the ſame quantity as thoſe of the ox.

The reſults of a general analyſis of the bones of the ox were:

Dry gelatine - - - -	51	0
Phoſphate of lime - - -	37	7
Carb. of lime - - - -	10	0
Phoſphate of magneſia - -	1	3
	100	0

The preſence of phoſphate of magneſia in the bones of animals, and its total abſence in thoſe of man, calls upon the phyſiologiſt to point out the ſource whence this ſalt is derived in the former, and why it is not met with in the latter. That it forms a conſtituent part of the food of both, we have proved elſewhere; that phoſphate of lime enters into the compoſition of wheat, barley, oats, peas, &c. Why then is it not to be found in the bones of men? The nature of the human urine may perhaps aſſiſt in explaining this problem. We have proved that the urine of men contains phoſphate of magneſia, and that the urine of animals is free from it. This ſalt is therefore ejected in man, by the kidneys; it therefore does not enter into the compoſition of bones; moreover the urinary calculi of man frequently contain phoſphate of ammonia and magneſia, but no ſuch ſalts are ever found in the ſtony concretions of the inteſtinal canal.

IX.

On the Nature of Oxigen, Hidrogen, Caloric, &c. as deduced from Galvanic Experiments. By A CORRESPONDENT.

On the exiſtence of oxigen.

SINCE we are told that oxygen is one of the moſt eſſential ſubſtances in the production of the moſt ſtriking phenomena of nature; that heat and light reſult from its change of combination, nay, that animal life is dependant on it, it becom[illegible] a matter of very great utility to inveſtigate its nature, much more to prove whether it has any exiſtence. It is well known that this ſubſtance, which has now ſo long and ſo generally been admitted, has never yet been exhibited except in combination, and that the evidence of its exiſtence in combination has never been more than preſumptive. It appears from ſome late experiments to be poſſible to ſubſtitute known for unknown principles, and to relieve ſcience from thoſe agents which are merely hypothetical.

Oxigen and hidrogen taken to be water with the power from each reſpective end of the galvanic wire.

The wires from the galvanic combination of metals produce different effects when placed in the ſame veſſel of water. One produces inflammable air, the other vital air. When the circuit is made by the human body, a ſhock ſimilar to an electrical one is perceived. It therefore appears that thoſe powers which thus affect the human body, change water into inflammable and vital air. One wire always produces one air, and the other wire another air from water. Is it not therefore philoſophical to refer the production of one of theſe airs to the power proceeding from one end of the pile and water; and the production of the other air to the power proceeding from the other end of the pile and water? In this experiment we are made ſenſible of no other principle, power, or ſubſtance than the above-mentioned. Why ſhould we therefore have recourſe to two hypothetical ſubſtances, oxygen and hydrogen, which have never in any experiment been made ſenſible to us? Is it not philoſophical to refer phenomena to cauſes which are objects of our ſenſes, rather than to account for them by agents which are merely hypothetical?

Developement of the facts and inferences.

Under certain circumſtances water is converted into two airs, which airs have peculiar properties; in the galvanic experiment we are made acquainted with no other agent but thoſe powers which are elicited by the particular arrangement of metals, and theſe powers we are made ſenſible of; they produce different effects on various ſubſtances, and therefore I contend that theſe powers are different agents: for the ſame powers under the ſame circumſtances ſhould produce the ſame effects. The zinc ſide of the galvanic arrangement produces vital air, whilſt the copper ſide produces inflammable air.—Does it not appear from this experiment that there are other cauſes beſides caloric that give aeriform elaſticity to bodies? And do not the two powers of the pile here ſeem to be real principles? Each of them produces a real and decided effect on water. At all events we have not in this experiment any reaſon for aſſerting that water is a compound body, formed of two diſtinct and ſolid ſubſtances, oxygen and hydrogen. I do not mean at preſent to inquire whether negative electricity be a mere negation or not. We know that it is as much a cauſe of repulſion as what is called poſitive electricity, and that in experiments of a different kind from the one we are now conſidering. When a ſubſtance has in one inſtance been clearly proved to be formed of certain principles, it is conſiſtent with philoſophical accuracy to refer in all other inſtances to the ſame principles as the cauſes of the production of ſuch ſubſtance. Inflammable air therefore is water rendered aeriform by negative electricity or galvaniſm; and vital air is water rendered aeriform by poſitive electricity or galvaniſm. This is nearly the enunciation of the fact, and I contend that in this, and in all the reaſonings reſpecting water and fire, we have no occaſion for the two hypothetical principles, oxygen and hydrogen.

The effect not cauſed by caloric.

In the above experiment with the pile of Volta, it appears that it is not caloric which cauſes the elaſtic aeriform ſtate of either the vital or the inflammable air, at leaſt we are not made ſenſible of it. If it were caloric that proceeded from the two wires of the pile, why ſhould each wire uniformly produce the ſame air, and one different from the other? There ſhould be ſome reaſon why the ſolid baſe hydrogen attaches itſelf to the caloric of the one wire, and why the ſolid baſe oxygen is always ſo ready to enter into combination with the caloric of the other wire.

Compound nature of heat.

Scheele has aſſerted the compound nature of the matter of heat, and that all inflammable bodies contain a principle of inflammability, which principle, by combining with his igneous air, produces the phenomena of combuſtion. It does not appear that this opinion has been controverted by any deciſive fact. We now know that ſome principle beſides caloric is neceſſary in the production of one inflammable body, namely, of inflammable air; which principle, by combining with vital or igneous air, produces combuſtion. The re-produced water is common to both.

Generation of fire.

I think upon a further proſecution of this inquiry, it will appear, that fire is generated during combuſtion, that it is fire alone, (that is, the principle which cauſes the ſenſation of heat) that cauſes all the phenomena of combuſtion, and that it is, as Scheele obſerves, the water of his igneous air which forms the additional weight of bodies after they are burned. I know of no chemical fact that contradicts this explanation, in which no new ſubſtance that is not ſenſibly diſcovered is introduced.

A CORRESPONDENT.

X.

Experiments on the Yolk of Wool, followed by ſome Conſiderations on the Cleanſing and Bleaching of Wool. *By* CIT. VAUQUELIN.*

Yolk of wool.

SEVERAL philoſophers have thought that the yolk of wool was a fatty matter; others from its diſſolving in water could not adopt the ſame opinion. Chemical analyſis alone could decide this queſtion, and this is what I propoſed to myſelf in the work, the reſult of which I now offer.

Action of water upon wool.

1ſt. Water deprives wool of much of its colour, and the liquid acquires colour, odour and taſte.

2d. The waſhings of the wool is milky like an emulſion of gum-reſin, and paſſes through paper with difficulty.

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* From the Ann. de Chimie, Fructidor, An. XI. No. 141.

3d. In time it gives a depoſition of ſand, carbonate of lime, and ſeveral other foreign bodies; it lathers by agitation and heat like a ſolution of ſoap.

Aqueous ſolution and extract.

4th. The water with which wool has been waſhed, filtered and evaporated, yields a brown extract, thick like a ſyrup, of an acrid, ſalt, and bitter taſte: in this ſtate it ſtill retains its peculiar odour.

Extract expoſed to alcohol.

5th. Alcohol, applied to this extract, diſſolves a part which communicates a reddiſh-brown colour to it: if the alcohol be ſeparated from this ſubſtance by evaporation, it aſſumes the form of a tranſparent, thick, and viſcous honey.

To acids.

The following are ſome of the properties which it offered:

1ſt. It diſſolves eaſily in water, and its ſolution is ſpeedily coagulated by the acids, which ſeparate a fat ſubſtance from it, inſoluble in water. The matter thus ſeparated by the acids, collects very ſlowly; its colour is yellowiſh. The acids, as will be ſeen lower, retain a great quantity of it in ſolution, which gives them a reddiſh brown colour. By evaporation, the greateſt part of this ſubſtance, diſſolved by the acids, is depoſited in the form of a black bitumen, and ſalts are obtained with baſe of potaſh and of lime. The greaſy matter is ſo adherent to theſe ſalts that they cannot be obtained in a ſtate of purity and whiteneſs, until after ſeveral calcinations and ſolutions.

It yields ſalts with baſe of lime and potaſh:

Acetous acid, and muriatic acid.

At the ſame time that the acids precipitate this fat matter, they drive off a certain quantity of acetous acid, very diſtinguiſhable by its odour. Concentrated ſulphuric acid blackens the inſpiſſated yolk, and diſengages ſome vapours of muriatic acid.

Lime-water.

2d. Lime-water renders the ſolution of the yolk turbid and milky, but it does not form a coagulum in it as in a ſolution of common ſoap.

Cauſtic alkalis and quick-lime.

3d. Cauſtic alkalis or quick-lime do not demonſtrate the preſence of ammonia.

Nitrate of ſilver.

4th. Nitrate of ſilver produces a yellow precipitate in it, which attaches itſelf to the ſides of the veſſel, like a *fat ſubſtance*. Great part of this precipitate is diſſolved in nitric acid.

The inſoluble part is ſtill ſalt.

Is not entirely ſoluble in water after having been treated with alcohol.

The part of the yolk which is inſoluble in alcohol has ſtill a ſalt taſte, but leſs diſtinct than the part which is ſoluble in this re-agent. After having been thus treated with alcohol, it does not entirely re-diſſolve in water; there remains a glutinous matter, Contains an alkaline carbonate.

Action of reagents.

matter, of a grey colour, with which the acids produce a pretty briſk efferveſcence, which ſhows the preſence of an alkaline carbonate. The portion which retains its ſolubility in water communicates a reddiſh colour and a ſaline taſte to this fluid; its ſolution is not diſturbed by the acids, as it was before having been treated by alcohol. Cauſtic alkalis do not diſengage any ammonia; the muriate of barites forms a very abundant depoſition in it, the greateſt part of which is ſoluble in water: the nitrate of ſilver alſo occaſions a precipitate in it, which diſſolves partly in nitric acid. Alcohol precipitates this matter in the form of a mucilage, which is depoſited quickly.

Nitrate of iron being mixed with the ſolution of this ſubſtance, formed a brown precipitate in it, and at the end of ſome days, the liquor furniſhed a pretty large quantity of nitrate of potaſh.

The yolk being decompoſed by dilute ſulphuric acid, and the liquor filtered, it blackened by evaporation, exhaled vapours of ſulphuric acid, and became carbonaceous, as the concentration of the ſulphuric acid took place. The reſidue being afterwards waſhed with water, and the ſolution ſuitably evaporated, yielded cryſtals of neutral ſulphate of potaſh, but a good deal remained in the ſolution on account of the ſuperabundant acid which brought it to the ſtate of an acidulous ſalt: by a longer evaporation, this ſalt cryſtallizes in needles and plates of a pearly white.

Yields ſulphate of lime by ſulphuric acid;

During the courſe of theſe ſucceſſive evaporations, another ſpecies of ſalt was offered, in the form of flattened needles, of a ſattiny white, and without any ſenſible taſte.

This ſalt examined with care, appeared to me to be only ſulphate of lime; it however differed from it in ſome reſpects: for example, it melts much more readily by the flame of the blow-pipe into a globule, tranſparent while it is in fuſion, and which becomes opaque by contracting: it is alſo much more ſoluble in water, and nevertheleſs does not contain the acid in exceſs; as I have ſatisfied myſelf. Its ſolution in water precipitates muriate of barites and oxalate of ammonia abundantly: one of theſe precipitates is ſulphate of barites, and the other oxalate of lime. Neither lime-water nor ammonia diſturb its ſolution. It appears therefore that this ſalt is a modification of ſulphate of lime, which is probably produced by the proportion of its elements. It may alſo be poſſible that this ſalt ſtill contains ſome portions of fat matter, which, by decompoſing the ſulphate of lime, and forming a little ſulphuret would facilitate the fuſion. I regret my not having had a ſufficient quantity of this ſalt, to examine its properties more minutely.

Acetic acid;

The yolk diſſolved in water, filtered, and inſpiſſated, having been diſtilled with dilute ſulphuric acid, furniſhed a liquor in which I eaſily recognized acetic acid, by its odour, its taſte, and the properties of the ſalts which it formed with different baſes, particularly with lime and potaſh.

Thus the yolk contains acetic acid, which without doubt is combined with part of the potaſh.

and muriatic acid.

It [illegible] contains muriate of potaſh, for, with the ſolution of ſi[illegible] forms an abundant precipitate, which is not entirely ſoluble in nitric acid; and, by diſtillation with ſulphuric acid, it gives ſenſible indications of muriatic acid, which is mixed with the acetic acid.

The yolk evaporated to dryneſs, and ſtrongly heated in a ſilver crucible, ſwells, chars, and exhales fetid ammoniacal vapours; afterwards oily fumes ariſe which take fire, and when the greateſt part of the oil is diſſipated, it reddens, and enters into quiet fuſion. If, at this moment, it be poured on a marble, it yields a ſubſtance which contracts by cooling, of a greyiſh colour, and a very cauſtic alkaline taſte: if this ſubſtance be afterwards diſſolved in water, there only remains an infinitely ſmall quantity of carbonaceous matter, and, by evaporation, the liquor yields a true potaſh ſlightly carbonated.

Recapitulation of the component parts;

It reſults from theſe experiments that the oil or greaſe, whoſe preſence in the yolk has been demonſtrated by means of the acids, is combined in it with potaſh, in the ſtate of a true animal ſoap; that, beſides, there is a portion of carbonate of potaſh in exceſs; ſince the acids produce a pretty briſk frothy effervefcence in the concentrated ſolution of the yolk. In addition to the ſubſtances which I have juſt mentioned, the yolk contains a certain quantity of animal matter; for, by diſtillation, it gives very ſenſible traces of ammoniac, and an oil whoſe fetid odour reſembles thoſe furniſhed by animal matters.

The yolk is therefore formed, *1ſt,* Of a ſoap with a baſe of potaſh, which makes the greateſt part; *2d,* Of a ſmall quantity of carbonate of potaſh; *3d,* Of a perceptible quantity of acetate of potaſh; *4th,* Of lime, whoſe ſtate of combination

I am unacquainted with; *5th*, Of an atom of muriate of pot-aſh; *6th*, finally, Of an animal matter to which I attribute the peculiar odour of the yolk.

which are not accidental.

I am of opinion that all theſe matters are eſſential to the nature of the yolk, and are not found in it by accident; for I have conſtantly found them in a great number of ſamples, as well of Spaniſh wool as of French.

I do not here ſpeak of the other matters, inſoluble in water, which are alſo met with in wool, ſuch as the carbonate of lime, ſand, and filth of every ſort, theſe being evidently accidental.

Are they the products of cutaneous tranſpiration?

It remained now to enquire if all the matters in the yolk were the product of cutaneous tranſpiration, accumulated and thickened in the wool, or if they were taken up in the folds and other places in which the ſheep lie. It is very certain that all the elements fit for the formation of the matters contained in the yolk, are found in the excrements of theſe animals, and in the vegetables which ſerve them for litter. Nevertheleſs, I could not believe that all of it was the effect of dung; on the contrary, I am of opinion that the humour of the tranſpiration is the principal ſource of it.

The analyſis of the dung offers nothing certain in this reſpect, becauſe the matters found in it may have been depoſited there by the ſheep themſelves.

In what ſtate are they emitted by the ſkin?

But admitting that the principles of the yolk ariſe from the cutaneous tranſpiration, which is very probable, are theſe matters emitted by the body of the animal in this ſtate, and do they not experience ſome change while they remain in the wool? This is a queſtion on which it is difficult to decide poſitively; we can only preſume that changes are produced in it, as in all very complex ſubſtances deprived of motion, of which, in the preſent caſe, we neither know the cauſe nor the manner.

Waſhing the wool in running water is not enough to cleanſe it.

The yolk, as we have ſeen above, being a true ſoap, ſoluble in water and alcohol, it would ſeem that nothing better can be done for ſcowering the wool than to waſh them in running water. But I ſhould obſerve that there is a ſmall quantity of fat matter in the wool, which is not in combination with the alkali, and which, remaining attached to the wool, keeps it a little glutinous *(poiſſeux)*, notwithſtanding the moſt careful waſhing.

But if the wool be put into buckets, and only as much water as will moiſten it poured in, and if it be ſuffered to remain ſome time in this bath, preſſing it often, it ſcours much better, and becomes much whiter afterwards, by waſhing in running water.

Putrid urine does not promote the ſcouring.

The ſcourers have a cuſtom of macerating the wool in putrefied urine, and it is generally believed that it is the ammonia which is developed that effects the ſcouring; but I have ſome reaſon to think that this alkali is of no value. This effect is rather owing to the yolk itſelf, or to ſome other principle of the urine, to the urée, for example. The following are the grounds of my opinion in this reſpect; I put wool waſhed in running water into a mixture of ſal ammonia and common potaſh; the mixture had a ſtrong ſmell of ammonia, and nevertheleſs the wool was in no reſpect cleanſed, becauſe this alkali does not form, or at leaſt with great difficulty, a ſaponaceous combination with the greaſy matter of wool. From theſe obſervations, therefore, I believe putrid urine to be nearly uſeleſs in the ſcouring of wool, at leaſt as far as reſpects its ammonia.

Freſh urine would precipitate the greaſe.

Though the utility of putrified urine be in ſome degree doubtful, it is, on the contrary, very certain that freſh urine would be greatly injurious to the propoſed object, for the ſoap contained in the yolk would inconteſtibly experience a decompoſition by the acid of the urine, which would precipitate the greaſe on the wool.

As would water containing earthy ſalts.

I ſuſpect that the ſame effect would take place from waſhing the wool in water containing earthy ſalts, which are known to decompoſe alkaline ſoaps. For which reaſon it is always prudent to employ the pureſt water which can be procured for this purpoſe.

Soap-ſuds the beſt menſtruum.

This is not the caſe with ſoap-ſuds, which accompliſh the ſcouring of wool perfectly, at the ſame time giving it more whiteneſs. If, therefore, after having waſhed the wool in running water till it loſes no more, it be ſuffered to macerate for a few hours in only one twentieth of its weight of ſoap diſſolved in a ſufficient quantity of warm water, ſqueezing it often, it will be entirely purged of the ſmall portion of greaſe which ſtill adhered to it, and will then have a ſoftneſs and degree of clearneſs which it could not have had without this operation.

The yolk exerciſes an action on the uncombined greaſe.

Danger of continuing its action too long; or of uſing ſtrong ſoap-ſuds.

The yolk itſelf, when a little concentrated, as I have already mentioned, has an efficacious action on the portion of greaſe which is not in a ſaponaceous ſtate; for I have found that, in putting to the wool only the quantity of water neceſſary to cover it, it ſcours better, particularly with a little heat, than when it is waſhed in running water. But I alſo found that, when wool has remained too long in its own yolk, it ſwells, ſplits, and loſes its ſtrength: this effect alſo takes place with ſoap-ſuds which are too ſtrong.

The acrimony of the yolk probably hurtful to the living animal.

Waſhing recommended.

Since the ſolution of the yolk occaſions this ſwelling and ſplitting of the wool, is it not poſſible that this accident may happen on the ſheep's back, particularly in hot, moiſt ſeaſons, or when they are ſhut up in folds in which the litter is not often enough renewed? Nor would it be impoſſible that the acrimony of the yolk ſhould occaſion an irritation in their ſkins, and, by that means, be the cauſe of ſome of the diſorders to which this organ is liable in theſe animals, which muſt principally happen in hot and damp weather: fortunately, in theſe ſeaſons, they are from time to time expoſed to rains which waſh them, and carry off at leaſt a portion of this matter. On this ſubject I cordially agree with thoſe who think that waſhing ſheep in hot and dry weather, would be uſeful to their health and the quality of their wool.

Loſs of weight by ſcouring.

The loſs experienced by ſcouring wool is very variable; the greateſt I met with was 45 per cent. and the leaſt 35; it it is true, thoſe which I waſhed were very dry. This loſs is not wholly owing to the yolk; the humidity, the earth, and the filth of every ſpecies, alſo contribute to it.

Bleaching of ſcoured wool.

I have made ſome attempts to bleach ſcoured wool, but I confeſs that they have not been carried ſo far as they ought to have been. I have remarked, generally, that thoſe which had been waſhed with ſoap-ſuds whitened better, by every method, than thoſe which had not. Sulphureous acid diſſolved in water whitened it pretty well, but it did not deſtroy the yellow colour which the wool, growing in the groin and under the fore-legs of the ſheep, had contracted. In liquid ſulphureous acid the wool acquires the property of crackling between the fingers like brimſtoned ſilk, and, at the ſame time, contracts a very powerful fetid ſmell, which is not diſſipated in a long time.

I did not try the vapour of burning ſulphur, but all the world knows that it whitens wool well, and that the woollen manufacturers uſe it to give the finiſhing degree of whiteneſs to their goods. Of all the methods which I tried, I found none better for bleaching the wool, than expoſing it, on the graſs, to the dew and the ſun, after being well ſcoured with weak ſoap-ſuds: the yellow ſpots of that from the groin, however, were not entirely deſtroyed; they had only diminiſhed in intenſity.

XI.

Copy of a Letter from Mr. CUTHBERTSON *to Dr.* PEARSON, *communicating an important and curious diſtinguiſhing Property between the Galvanic and Electric Fluids. Communicated by Dr.* PEARSON.

To Dr. PEARSON:

DEAR SIR,

Galvanic deflagrations.

I THINK it right to inform you, that yeſterday evening I reſumed the experiments with the galvanic batteries; the reſult was—

1. Charcoal was deflagrated and ignited for about one inch in length.

2. Iron wire $\frac{1}{40}$ inch diameter, was melted into a ball $\frac{1}{16}$ inch diameter.

3. Platina wire $\frac{1}{100}$ inch diameter, was melted into a ball $\frac{1}{10}$ inch diameter.

4. Braſs wire $\frac{1}{20}$ inch diameter, $\frac{3}{4}$ inch in length was ignited.

5. Ditto $\frac{1}{16}$ inch diameter, was red-hot at the extremity.

6. Iron wire $\frac{1}{150}$ inch diameter, was red-hot for 16 inches in length.

7. Ditto 12 inches, deflagrated and melted into a ball.

8. Ditto ſix inches in length, were deflagrated.

9. Ditto 8 inches in length, were ignited.

Two troughs, each trough containing 30 pair of plates ſix inches ſquare, were uſed for the firſt ſeven experiments, and *one* of theſe troughs only for the two laſt experiments.

Galvanic deflagrations.

The four laſt experiments prove, I think, that *double quantities of galvanic fluid only burn double* lengths of wire, and not the ſquare, as electrical diſcharges do*.

I am, with the greateſt reſpect,
Sir,
Your very humble ſervant,
JOHN CUTHBERTSON.

Poland-Street, Soho,
March 27, 1804.

XII.

Letter from a Correſpondent, containing an Obſervation of the ſpontaneous Inflammation of Paper in Nitric Acid Gas.

To Mr. NICHOLSON.

DEAR SIR, *London, May* 22, 1804.

Paper inflamed in nitrous gas.

THAT ſeveral different inflammable bodies, while in a ſtate of inflammation or of ignition, burn with an enlarged flame, and continue ignited when immerſed in nitric acid gas, is, I ſuppoſe, commonly known; but that paper itſelf would take fire and flame moſt beautifully in this gas, and at not a very elevated temperature, has not, that I recollect, been already obſerved. By the following accidental circumſtance this phenomenon was ſeen this morning in the public lecture-room, while reading on the ſubject of nitrous acid. In putting together the different parts of the Wolfe's apparatus, having ready only a bent tube much ſmaller than the lateral aperture of the globe condenſing receiver, I filled up that aperture, partly with a piece of writing paper which projected into the receiver, and partly with almond paſte. Soon after the acid had begun to diſtil, and while the apparatus was filled with reddiſh coloured nitrous acid gas, I was ſurprized by the burſting forth of flame from the paper, which was conſumed by it in leſs than a minute, without cracking, as I expected, the receiver. I think it unneceſſary to make any comments, or give the *rationale* of this fact.

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* It is not ſaid whether the two troughs were uſed collaterally or longitudinally. Electrical jars may be conſidered as being always combined in the former mode. This ſubject ſeems to require compariſon with the facts given in Mr. Wilkinſon's letter in our Journal, Vol. VII. p. 207; but the communication came too late for me to offer any remarks upon it. W. N.

Always your's faithfully,

AMICUS.

XIII.

Deſcription of a Jib on a new Conſtruction; by Mr. J. BRAMAH, *Engineer. Communicated by the Inventor.*

Deſcription of a jib on a new conſtruction.

JIBS of the uſual conſtruction turn on two ſolid gudgeons. The rope by which the goods are raiſed, paſſes over the upper gudgeon, and is confined between two ſmall vertical rollers, in order that it may conſtantly lead fair with the pulley or ſheave at the extremity of the jib. According to this conſtruction, whenever the crane turns round its axis, the rope is bended ſo as to form an angle more or leſs acute, which cauſes a great increaſe of friction, and produces a continual effort to bring the arm of the jib into a parallel poſition to the inner part of the rope. Theſe inconveniences may appear to be trifling on paper, but in actual practice they are of no ſmall importance, for they neceſſarily imply a much greater exertion of power in raiſing goods, and the application of a conſtant force to keep the jib in the poſition that may be requiſite; while the partial ſtreſs which is exerted on only a few ſtrands of the rope, when bended into an acute angle, infallibly deſtroys it in a very ſhort time.

The ſimple conſtruction exhibited in Plate V. obviates all theſe defects, and at the ſame time poſſeſſes the very deſirable property of permitting the jib of what is termed a campſhut or landing crane, wholly to revolve round its axis, and to land goods at any point of the circle deſcribed by the arm of the jib.

It conſiſts in perforating the axis or pillar of the crane, and in conducting the rope through this perforation by means of an additional pully fixed on the top of the arm of the jib.

The

Deſcription of a jib on a new conſtruction.

The nature of the contrivance cannot fail to be ſufficiently underſtood by an inſpection of the figures; the one of which repreſents a jib attached to the wall of a warehouſe, the other a campſhot or landing crane fixed on the edge of the wharf. Each of theſe jibs turns on a perforated axis or pillar. The rope proceeds from the goods which are hoiſted, through a pully fixed as uſual at the extremity of the jib; it then paſſes over another pully fixed at the oppoſite extremity of the jib, and is, by this pully conducted through the perforated axis or pillar to a third pulley; whence it is immediately directed to the crane by which the weight is elevated.

It is almoſt unneceſſary to ſtate that the lower axis is uſually fixed in an oil box, and that friction rollers are applied to the axis wherever the circumſtances may render it neceſſary.

The importance of this improvement, in an article of ſuch extenſive uſe, muſt be evident even to thoſe who are the leaſt acquainted with the ſubject. Mechanics who are aware that ſimplicity of conſtruction and certainty of effect are among the moſt valuable characters to be ſought in engines, will moſt probably obſerve this crane with pleaſure; and the advantages to the community at large muſt be meaſured by the convenience and ſaving of labour it is calculated to afford.

XIV.

A Memoir concerning the Faſcinating Faculty which has been a[illegible]cribed to the Rattle-Snake, and other American Serpents. [illegible] BENJAMIN SMITH BARTON, M. D. *From the American Tranſactions,* Vol. IV.

(Concluded from Page 62.)

Other ſnakes (particularly the black-ſnake which is not poiſonous) are ſaid to charm.

SECONDLY. It is a fact well known in this country, that the rattle-ſnake is not the only kind of ſerpent that is ſaid to be endued with the faculty of faſcinating birds, ſquirrels, and other animals. As far as my inquiries have extended, it does not appear to me that, in general, the rattle-ſnake is thought to have ſo large a portion of this faculty as ſome other ſpecies of ſerpents. Of this, at leaſt, I am certain, that perſons reſiding in our country-ſituations tell us many wonderful tales of the bewitching eyes of the black-ſnake, the coluber conſtrictor of Linnæus, as they do of the boiquira, or rattle-ſnake. Now let it be ſuppoſed, for a minute, that the poiſon of this latter ſerpent, when thrown into the body of a bird, a ſquirrel, &c. is capable of producing, in theſe animals, thoſe piteous cries, thoſe ſingular movements, thoſe tremulous fears, which are mentioned by Kalm, by de la Cépède, and by other writers,—in what manner are we to account for the ſimilar cries, movements, and fears, in thoſe birds which are frequently ſeen under the faſcinating influence of the black-ſnake? For we Americans all know, that the bite of the black-ſnake is perfectly innoxious. This, indeed, is alſo the caſe with the greater number of the ſpecies of ſerpents that have, hitherto, been diſcovered in the extenſive country of the United States. And yet almoſt every ſpecies of ſerpents is ſuppoſed to be endued with the power of faſcinating ſuch animals as it occaſionally devours.

and conſequently the ſuppoſition of poiſonous influence is unfounded.

Theſe facts, and this mode of reaſoning, certainly involve, in ſome difficulty, Mr. de la Cépède, and thoſe writers who eſpouſe his opinion, which I have examined, under the firſt head of my objections. An attempt is made to account for the imaginary faſcinating faculty of the ſerpent from the powerful influence of a ſubtile poiſon. But, upon inquiry, it is found, that the power of bewitching different animals is not an excluſive gift of thoſe ſerpents which nature has provided with envenomed fangs: it is a gift which as extenſively belongs to that more numerous tribe of our ſerpents, whoſe bite is innocent, and whoſe creeping motion is their only poiſon *. Theſe

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* If there is any impropriety in this mode of expreſſion, the impropriety has its ſource in my feelings, with reſpect to the ſerpents. Perhaps, no man experiences the force and the miſeries of this prejudice in a greater degree than I do. It is the only prejudice which, I think, I have not ſtrength to ſubdue. As the natural hiſtory of the Serpents is a very curious and intereſting part of the ſcience of zoology; as the United-States afford an ample opportunity for the farther improvement of the hiſtory of theſe animals, and as I have, for a long time, been anxious to devote a portion of my leiſure time to an inveſtigation of their phyſiology, in particular, I cannot but exceedingly regret my weakneſs and timidity, in this reſpect. I had meditated a ſeries of experiments upon the reſpiration, the digeſtion, and the generation of the ſerpents of Pennſylvania. But, I want the fortitude which it is neceſſary to poſſeſs in entering on the

Theſe objections will, I am perſuaded, be ſufficient to convince every unprejudiced reader, that the ſyſtem of explanation offered by Mr. de la Cépède is unfounded in facts; and, conſequently, that the problem ſtill remains to be ſolved, in another way.

Profeſſor Blumenbach admits the fact; but aſcribes it to a moral cauſe.

Among the number of ingenious men who have amuſed themſelves with ſpeculations on the ſubject of this memoir, and who, rejecting the commonly received notion of the exiſtence of a faſcinating power in the rattle-ſnake, have attempted to explain the phenomenon upon other principles, it is with pleaſure I recognize the reſpectable Profeſſor Blumenbach, of Gottingen. This gentleman, in a late publication, ſpeaking of the rattle-ſnake, makes a few remarks on the faſcinating faculty which has been aſcribed to this reptile. Theſe remarks I ſhall tranſlate at length.

Toads, hawks, &c. are ſaid to have the power of faſcinating by the eye.

"That ſquirrels, ſmall birds, &c." ſays he, "voluntarily fall from trees into the jaws of the rattle-ſnake, lying under them, is certainly founded in facts: nor is this much to be wondered at, as ſimilar phenomena have been obſerved in other ſpecies of ſerpents, and even in toads, hawks, and in cats, all of which, to appearance, can under particular circumſtances, entice other ſmall animals, by mere ſtedfaſt looks. Here the rattles of this ſnake (the rattle-ſnake) are of peculiar ſervice; for their hiſſing noiſe cauſes the ſquirrels, whether impelled by a kind of curioſity, miſunderſtanding, or dreadful fear, to follow it, as it would ſeem, of their own accord. At leaſt," continues Mr. Blumenbach, "I know from well-informed eye witneſſes, that it is one of the common practices among the younger ſavages to hide themſelves in the woods, and by counterfeiting the hiſſing of the rattle-ſnake to allure and catch the ſquirrels."*.

the taſk. Inſtead of ſlowly and cautiouſly diſſecting and examining their ſtructure and their functions, with that attention which the ſubject merits, I am more diſpoſed, at preſent, to obey the injunction of the Mantuan poet, in the following beautiful lines:

———Cape ſaxa manu: cape robora, paſtor,
Tollentemque minas et ſibila colla tumentem
Dijice: jamque fuga tumidum caput abdidit alte,
Cum medii nexus, extremæque agmina caudæ
Solvuntur, tardoſque trahit ſinus ultimus orbes.

GEORG. Lib. iii. 420—424.

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* Handbuch der Naturgeſchichte, P. 253. Gœttingen: 1791.

I do not intend to take up much time in examining the foregoing explanation. I ſhall offer my objections to it, in as conciſe a manner as I can.

Examination of the facts ſtated by Blumenbach, which are conteſted.

Firſt. The faculty of faſcinating is by no means peculiar to the rattle-ſnake, but is attributed as extenſively to the black-ſnake, and other ſerpents, which are not furniſhed with the crepitaculum, or ſet of bells *, by which this ſerpent is ſuppoſed to be enabled to ring for its prey, when it wants it.

Secondly. Some perſons, who have ſeen the rattle-ſnake in the ſuppoſed act of charming, aſſure me that the reptile did not ſhake its rattles, but kept them ſtill. It is true, that Mr. Voſmäer's rattle-ſnake, already mentioned, continually ſhook its rattles.

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Thirdly. With regard to the practice of the young ſavages, ſpoken of by Mr. Blumenbach, I know nothing. I have inquired of Indians, and of perſons who have reſided for a conſiderable time, among the Indians, and they appear to be as ignorant of the circumſtance as I am myſelf. I am inclined to think that Mr. Blumenbach has been impoſed upon: or, perhaps, the following circumſtance may have given riſe to the ſtory. The young Indians put arrows, acroſs, in their mouths, and by the quivering motion of their lips upon the arrows, imitate the noiſe of young birds, thus bringing the old ones ſo near to them, that they can be readily ſhot at. In like manner, the Lanius Excubitor, or great ſhrike, hiding itſelf in a thicket, and imitating the cry of a young bird, often ſucceeds in ſeizing the old ones, which have been ſolicited, by the counterfeited noiſe, to the aſſiſtance of their young.

Ever ſince I have been accuſtomed to contemplate the objects of nature with a degree of minute attention, I have conſidered the whole ſtory of the enchanting faculty of the rattle-ſnake, and of other ſerpents, as deſtitute of a ſolid foundation. I have attentively liſtened to many ſtories, which have been related to me as proofs of the doctrine, by men whoſe veracity I could not ſuſpect. But there is a ſtubborn incredulity often attached to certain minds. In me it was ſtrong. The mere force of argument never compelled me to believe. I always ſuſpected, that there was ſome deficiency in the extent of obſervation, and the reſult of not a little attention to the ſubject has taught me, that there is but one

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* Serpent à ſonnette is the French name for the rattle-ſnake.

wonder

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wonder in the buſineſs ;——the wonder that the ſtory ſhould ever have been believed by a man of underſtanding, and of obſervation.

In conducting my inquiries into this curious ſubject, I thought it would be proper, and even neceſſary, previouſly to my forming a decided opinion, to aſcertain the two following points, viz. firſt: what ſpecies of birds are moſt frequently obſerved to be enchanted by the ſerpents? and, ſecondly, at what ſeaſon of the year has any particular ſpecies been moſt commonly ſeen under this wonderful influence? I was induced to believe that the ſolution of theſe two queſtions would ſerve as a clue to the inveſtigation of what has been long conſidered as one of the moſt myſterious operations in nature. I am perſuaded that I have not been miſtaken. Poſſibly, the credulous may not think as I do.

It is a curious circumſtance in the hiſtory of birds, that almoſt every ſpecies, in the ſame country at leaſt, has an almoſt uniform and determinate method of building its neſt, whether we conſider the form of the neſt, the materials of which it is conſtructed, or the place in which it is fixed *. Some obſervations on this ſubject are neceſſarily connected with the point under inveſtigation, in this memoir;—indeed, they are involved in the queſtion concerning the ſpecies of birds which have moſt generally been obſerved to be enchanted by the rattle-ſnake, &c.

Some birds build their neſts on the ſummits of the loftieſt trees; others ſuſpend them, in a pendulous manner, at the extremity of a branch, or even on a leaf †, whilſt others build

them

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* I do not mean, by this obſervation, to aſſert, that birds are neceſſarily impelled to conſtruct their neſts of the ſame materials, or to place them in the ſame ſituations; yet ſuch is the language of ſome writers on natural hiſtory, and on morals, who talk of the "determinate inſtinct" of animals, and who think it impoſſible that "animals of the ſame ſpecies ſhould any where differ." "The grouſe in America, we are told, perch upon trees; the hare burrows in the ground; and we have, in theſe inſtances, ſufficient reaſon to deny that the ſpecies of either is the ſame with thoſe of a like denomination, with which we are acquainted, in Europe." Theſe are the words of the celebrated author. See Dr. A. Ferguſon's Principles of Moral and Political Science, vol. i. p. 59 & 60. quarto edition.

† See a very intereſting account of the Motacilla ſutoria, or

Taylor-

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them on the lower branches, among buſhes, and in the hollows of decayed, and other trees. Many ſpecies, again, are content with the ground, laying their eggs, and hatching them, in the cavity of a ſtone, an excavation from the earth, among the graſs of fields and meadows, or in fields of wheat, rye, and other grains. Thus, to confine myſelf to our own country, the eagle, the vulture, the hawk, and other birds of this extenſive family, make choice of the loftieſt oaks, and other trees of our foreſts; the baltimore-oriole [*], commonly called, in Pennſylvania, the hanging bird, ſuſpends a beautiful neſt to the extremity of a branch of the Liriodendron [†], or ſome other tree; the migrating thruſh [‡], called robin, is content with the lower branches; the red thruſh [§], the cat-bird [||], the red-winged oriole, called the ſwamp-black-bird [¶], and many others build in the low buſhes; the wood-peckers [**], the blue motacilla (blue-bird) [††], the torchepot [‡‡], and others, build in the hollows of trees, the chattering plover [§§], and the whippoor-will [||||], take advantage of a hollow place in the ground, or in a ſtone, which the great lark [¶¶], the marſh-wren [***], &c. place their neſts in the graſs; and, laſtly, the partridge [†††] builds in the corn-fields.

Of all theſe birds, and of a great many others, thoſe which build their neſts upon the ground, on the lower branches of trees, and on low buſhes (eſpecially on the ſides of rivers, creeks, and other waters, that are frequented by different kinds of ſerpents), have moſt frequently been obſerved to be under the enchanting faculty of the rattle-ſnake, &c. Indeed, the bewitching ſpirit of theſe ſerpents ſeems to be almoſt entirely limited to theſe kinds of birds. Hence, we ſo frequently hear tales of the faſcination of our cat-bird, which builds its neſt in the low buſhes, on the ſides of creeks, and other waters, the moſt uſual haunts of the black-ſnake, and

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Tayler-bird, by my learned friend Mr. Pennant, in his Indian Zoology, pages 44, 45 & 46.

[*] Oriolus Baltimore.

[‡] Turdus migratorius.

[||] Muſcicapa carolinenſis.

[**] Pici

[‡‡] Sitta.

[||||] Caprimulgus.

[***] Motacilla Troglodytes?

[†] Liriodendron tulipifera.

[§] Turdus rufus.

[¶] Oriolus phœniceus.

[††] Motacilla Sialis.

[§§] Charadrius vociferus.

[¶¶] Alauda magna.

[†††] Tetrao virginianus.

other

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other ſerpents. Hence, too, upon opening the ſtomachs of ſome of our ſerpents, if we often find that they contain birds, it is almoſt entirely thoſe birds which build in the manner I have juſt mentioned.

This fact I had long remarked. It had made ſome impreſſion upon my mind before I had turned my attention to the ſubject of this memoir. Lately, when I came to take a view of the ſubject, the fact appeared to me to be of ſome conſequence. I ſhall now avail myſelf of it.

The rattle-ſnake ſeldom, if ever, climbs up trees*. He is

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* Some reſpectable writers aſſert, that the rattle-ſnake does climb trees, and that it does it with eaſe. Mr. de la Cépède is of this opinion. After telling us that this reptile lives upon worms, frogs, and hares, this naturaliſt proceeds: "il fait auſſi ſa proie d'oiſeaux & d'écureuils; car il monte avec facilité ſur les arbres, & s'y élance avec vivacité de branche en branche, ainſi que ſur les pointes des rochers qu'il habite, & ce n'eſt que dans la plaine qu'il court avec difficulté, & qu'il eſt plus aisè d'eviter ſa pourſuite." *Hiſtoire Naturelle des Serpens.* p. 409. At the concluſion of his account of the boiquira, or crotalus horridus, the eloquent author has run into the ſame error, in the following beautiful, though rather poetical, apoſtrophe. "Tranquilles habitans de nos contrées tempérèes, que nous ſommes plus heureux, loin de ces plages où la chaleur & l'humidité regnent avec tant de force! Nous ne voyons point un Serpent funeſte infecter l'eau au milieu de laquelle il nage avec facilité; les arbres dont il parcourt les rameaux avec viteſſe; la terre dont il peuple les cavernes; les bois ſolitaires, où il exerce le même empire que le tigre dans ſes déſerts brûlans, dont l'obſcurite livre plus ſûrement ſa proie à ſa morſure. Ne regrettons pas les beautés naturelles de ces climats plus chauds que le nôtre, leurs arbres plus touffus, leurs feuillages plus agréables, leurs fleurs plus ſuaves, plus belles: ces fleurs, ces feuillages, ces arbres cachent la demeure du Serpent à ſonnette." *Hiſtoire Naturelle des Serpens.* p. 419 & 420. I have been at ſome pains to diſcover whether the rattle-ſnake does climb up trees. The reſult of my inquiries is that it does not. Although I have had opportunities of ſeeing great numbers of rattle-ſnakes in the weſtern parts of Pennſylvania, &c. particularly in the vicinity of the river Ohio, I never ſaw one of them except on the ground. The black-ſnake I have often ſeen upon trees. I ought not, however, to conceal that in the ſummer of the laſt year, a Choktah-Indian told me, that the rattle-ſnake does climb trees and buſhes, *to a ſmall height.* He ſaid, that he had once ſeen one of theſe ſnakes upon a reed. I am not very willing

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is frequently, however, found about their roots, eſpecially in wet ſituations. It is ſaid that this reptile is often ſeen, curled round a tree, darting terrible glances at a ſquirrel, which after ſome time is ſo much influenced by theſe glances, or by ſome ſubtile emanation from the body of the ſerpent, that the poor animal falls into the jaws of its enemy. This ſtory is, I believe, deſtitute of foundation, though it is related by the good Cotton Mather*. The rattle-ſnake is, indeed, ſometimes ſeen at the root of a tree, upon the lower branches of which, at the height of a few feet from the ground, a bird or ſquirrel has been ſeen exhibiting ſymptoms of fear and diſtreſs. Is this a matter of any wonder? Nature has taught different animals what animals are their enemies; and although, as will be afterwards ſhewn, the principal food of the rattle-ſnake is the great frog, yet as he occaſionally devours birds and ſquirrels, to theſe animals he muſt neceſſarily be an object of fear. When the reptile, therefore, lies at the foot of a tree, the bird or the ſquirrel will feel itſelf uneaſy. That it will ſometimes run towards the ſerpent, then retire, and return again, I will not deny. But that it is irreſiſtibly drawn into the jaws of the ſerpent, I do deny: becauſe it is very frequently ſeen to drive the ſerpent from its hold; becauſe the bird or ſquirrel often returns, in a few minutes, to their

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willing to deny this Indian's ſtory: yet it is oppoſed to every information I have been able to procure from perſons well acquainted with the reptile of which I am ſpeaking. However, it is not impoſſible that where trees and buſhes grow very cloſe together, the ſnake may climb them *to a very ſmall height.* Moſt ſpecies of ſerpents move in a ſpiral manner: the rattle-ſnake moves ſtrait on; and this is the reaſon why he cannot climb trees. In the quotation which I have made from Mr. de la Cépède, another miſtake is involved. He ſpeaks of the agility with which the rattle-ſnake moves. This is not, however, merely the miſtake of Mr. de la Cépède. We find it in Piſo. Speaking of this reptile, our author ſays: "In triviis juxta ac deviis locis cernitur, tam celeriter proreptans ut volare videatur, idque velocius per loca ſaxoſa, quam terreſtria." *De India utriuſque re naturali et medica.* p. 274. Now the truth is that the rattle-ſnake is one of the moſt ſluggiſh of all our ſerpents. Linnæus was well informed, when he aſſerted that Providence had given "the Crotalus a very ſlow motion." See *Reflections, &c.* quoted p. 84 of this memoir.

* Philoſophical Tranſactions of the Royal Society, No. 339.

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habitations. Sometimes the bird or ſquirrel, in attempting to drive away the ſnake, approach too near to their enemy, and are bitten, or immediately devoured. But, from what will afterwards be ſaid, it will appear that theſe inſtances are not ſo common as is generally imagined.

My inquiries concerning the ſeaſon of the year, at which any particular ſpecies of birds has been ſeen under the faſcinating power of a ſerpent, afforded me ſtill more ſatisfaction. In almoſt every inſtance, I found that the ſuppoſed faſcinating faculty of the ſerpent was exerted upon the birds at the particular ſeaſon of their laying their eggs, of their hatching, or of their rearing their young, ſtill tender, and defenceleſs. I now began to ſuſpect, that the cries and fears of birds ſuppoſed to be faſcinated originated in an endeavour to protect their neſt or young. My inquiries have convinced me that this is the caſe.

I have already obſerved, that the rattle-ſnake does not climb up trees. But the black-ſnake and ſome other ſpecies of the genus coluber do. When impelled by hunger, and incapable of ſatisfying it by the capture of animals on the ground, they begin to glide up trees or buſhes, upon which a bird has its neſt. The bird is not ignorant of the ſerpent's object. She leaves her neſt, whether it contains eggs or young ones, and endeavours to oppoſe the reptile's progreſs. In doing this, ſhe is actuated by the ſtrength of her inſtinctive attachment to her eggs, or of affection to her young. Her cry is melancholy, her motions are tremulous. She expoſes herſelf to the moſt imminent danger. Sometimes, ſhe approaches ſo near the reptile that he ſeizes her as his prey. But this is far from being univerſally the caſe. Often, ſhe compels the ſerpent to leave the tree, and then returns to her neſt *.

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* Horace, though he has not, like his contemporary, Virgil, given any great proofs of his knowledge in natural hiſtory, appears to have known, full well, the anxiety of birds for the preſervation of their young:

> " Ut aſſidens implumibus pullis avis
> " Serpentium allapſus timet."
>
> **Epod. 1.**

The author of theſe two fine lines, had he lived in America, the land of faſcination, would, I am inclined to think, have diſbelieved, the whole ſtory. They would have been a clue to light and truth on this ſubject.

It

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It is a well known fact, that among ſome ſpecies of birds, the female, at a certain period, is accuſtomed to compel the young ones to leave the neſt; that is, when the young have acquired ſo much ſtrength that they are no longer entitled to *all* her care. But they ſtill claim ſome of her care. Their flights are awkward, and ſoon broken by fatigue. They fall to the ground, where they are frequently expoſed to the attacks of the ſerpent, which attempts to devour them. In this ſituation of affairs, the mother will place herſelf upon a branch of a tree, or buſh, in the vicinity of the ſerpent. She will dart upon the ſerpent, in order to prevent the deſtruction of her young: but fear, the inſtinct of ſelf-preſervation, will compel her to retire. She leaves the ſerpent, however, but for a ſhort time, and then returns again. Oftentimes, ſhe prevents the deſtruction of her young, attacking the ſnake, with her wing, her beak, or her claws. Should the reptile ſucceed in capturing the young, the mother is expoſed to leſs danger. For, whilſt engaged in ſwallowing them, he has neither inclination nor power to ſeize upon the old one. But the appetite of the ſerpent-tribe is great: the capacity of their ſtomachs is not leſs ſo. The danger of the mother is at hand, when the young are devoured. The ſnake ſeizes upon her: and this is the cataſtrophe, which crowns the tale of faſcination!

An attachment to our offſpring is not peculiar to the human kind alone. It is an inſtinct which pervades the univerſe of animals. It is a ſpark of the divinity that actuates the greater number of living exiſtences. It is a paſſion which, in my mind, at leaſt, declares, in language moſt emphatic, the exiſtence, the ſuperintendance, the benevolence, of a firſt great cauſe, who regards with partial and parental, if not with equal eyes the falling of a ſparrow and the falling of an empire.

Among the greater number of the ſpecies of birds, the attachment of the parent to the young is remarkably ſtrong. We have daily inſtances of this attachment among our domeſtic birds, and I believe, it is ſtronger among theſe birds in their wild ſtate: for there are ſome reaſons for ſuſpecting, that this amiable inſtinct is diminiſhed and weakened by culture *.

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* This queſtion will be examined in my memoirs upon the ſtorge, or affections, of animals.

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The inſtances which I have already mentioned, as well as a fact, which remains to be mentioned, point out, in a ſtriking view, the attachment of the mother-bird to her offspring. She often guards her neſt with the greateſt attention, fearful of the inſidious glide of the ſerpent. She endeavours to prevent the deſtruction of her eggs or young, by this enemy. When he has ſucceeded in obtaining them, ſhe attacks him either alone, or calls other birds to her aſſiſtance. We ought not to be ſurpriſed, that ſometimes ſhe falls a victim to her affection. For it is a well known fact, that ſome ſpecies of birds will ſuffer themſelves to be taken upon their neſts, rather than relinquiſh their young, or their eggs.

In the ſtudy of natural hiſtory, I am always happy to diſcover new inſtances of the wiſdom of providence, and new proofs of the ſtrong affections of animals. And for the diſcovery of ſuch inſtances of wiſdom, and ſuch proofs of affection, the contemplation of nature is an ample field. In the inſtances now before us, the ſtrength of the inſtinct of affection in birds is illuſtrated, in a ſtriking point of view; and I cannot help obſerving, that I feel an high degree of pleaſure in being able to do away, in ſome meaſure at leaſt, a prejudice, not leſs extenſive than it is unfounded, by bearing my ſlender teſtimony in favour of the exiſtence and the powerful dominion of a benevolent principle in animals.

The following fact was communicated to me, ſome time ſince, by our preſident, Mr. Rittenhouſe. I think it ſtrikingly illuſtrates and confirms the ſyſtem which I have been endeavouring to eſtabliſh. I relate it, therefore, with pleaſure, and the more ſo, as I have no doubt, that the authority of a cautious and enlightened philoſopher will greatly contribute to the deſtruction of a ſuperſtitious notion which diſgraces the page of natural hiſtory.

Some years ſince, this ingenious gentleman was induced to ſuppoſe, from the peculiar melancholy cry of a red-winged-maize-thief*, that a ſnake was at no great diſtance from it, and that the bird was in diſtreſs. He threw a ſtone at the place from which the cry proceeded, which had the effect of driving the bird away. The poor animal, however, im-

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* Commonly called, in Pennſylvania, the Swamp-Black-bird. It is the Oriolus phœniceus of Linnæus.

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mediately returned to the ſame ſpot. Mr. Rittenhouſe now went to the place where the bird alighted, and, to his great aſtoniſhment, he found it perched upon the back of a large black-ſnake, which it was pecking with its beak. At this very time, the ſerpent was in the act of ſwallowing a young bird, and from the enlarged ſize of the reptile's belly it was evident, that it had already ſwallowed two or three other young birds. After the ſnake was killed, the old bird flew away.

Mr. Rittenhouſe ſays, that the cry and actions of this bird had been preciſely ſimilar to thoſe of a bird which is ſaid to be under the faſcinating influence of a ſerpent; and I doubt not that this very inſtance would, by many credulous perſons, have been adduced as a proof of the exiſtence of ſuch a faculty. But what can be more evident than the general explanation of this caſe? The maize-thief builds its neſt in low buſhes, the bottoms of which are the uſual haunts of the black-ſnake. The reptile found no difficulty in gliding up to the neſt, from which, moſt probably in the abſence of the mother, it had taken the young ones. Or it had ſeized the young ones, after they had been forced from the neſt, by the mother. In either caſe, the mother had come to prevent them from being devoured.

We are well acquainted with the common food of the rattle-ſnake. It is the great-frog[*] of our rivers, creeks, and other waters. The ſnake lies inſidiouſly in wait for his prey, at the water-edge. He employs no machinery of enchantment. He truſts to his cunning and his ſtrength.

A very ingenious[†] friend of mine, who has devoted conſiderable attention to the natural hiſtory of the rattle-ſnake, and who has diſſected many of them, aſſures me, that he never ſaw but one inſtance in which a bird was found in the ſtomach of this reptile, and this bird was the chewink, or ground-robin[‡]. In another inſtance, he ſaw a ground-ſquirrel[§] taken out of one of theſe reptiles. In every other caſe, ſo long as the food retained enough of the form to be diſtinguiſhed, the ſtomach was found to contain the great-frog, which I have mentioned.

[*] Rana ocellato of Linnæus.

[†] Timothy Matlack, Eſq.

[‡] This is the Fringilla erythrophthalma of Linnæus.

[§] The Sciurus ſtriatus of Linnæus.

Facts and obſervations reſpecting the power of faſcination aſcribed to ſnakes.

Another argument againſt the faſcinating power of the ſerpent-tribe ſtill remains to be conſidered.

It is natural to inquire, for what purpoſe nature has endued ſerpents with the ſuppoſed powers of faſcinating birds, and other animals? The anſwer to this queſtion is uniform. It is ſaid, the power is given that the ſerpents may obtain their food. Let us examine this opinion.

Admitting the exiſtence of this power, I ſhould have no heſitation in believing, that its uſe is what is here mentioned, though, indeed, it ought not to be concealed, that ſnakes are ſuppoſed, by ſome fooliſh people, to have the power of charming even children. And yet, I believe, there are no inſtances recorded of our American ſnakes devouring children. If, then, nature, in the immenſity of her kindneſs, had gifted the ſerpents with this wonderful power, we ſhould, at leaſt, expect to find that the common and principal food of theſe ſerpents was thoſe animals, viz. birds and ſquirrels, upon which this influence is generally obſerved to be exerted. This, however, is by no means the caſe.

As connected with this p[illegible] of my memoir, it will not be improper to obſerve, that [illegible]r ſerpents are the food of different kinds of birds. Ev[illegible]e rattle-ſnake, whoſe poiſon produces ſuch alarming ſymptoms in man, and other animals, is frequently devoured by ſome of our ſtronger and more courageous birds. As far as I can learn, the birds which moſt commonly attack and deſtroy this reptile, are the ſwallow-tailed hawk [*], and the larger kinds of owls. The owl often feeds her young with this ſnake, whoſe bones are frequently found in her neſt, at conſiderable heights from the ground. Even a hen has been known to leave, for a minute, her affrighted chickens, and attack, with her beak, a rattle-ſnake, the greater part of whoſe body ſhe afterwards devoured [†].

[*] Falco furcatus.

[†] It is commonly believed, that the rattle-ſnake is a very hardy animal; but this is not the caſe. A very ſmall ſtroke on any part of its body diſables it from running at all; and the ſlighteſt ſtroke upon the top of the head is followed by inſtant death. The ſkull-bone is remarkably thin and brittle; ſo much ſo indeed, that it is thought that a ſtroke from a wing of a thruſh or robin would be ſufficient to break it.

Facts and obſervations reſpecting the power of faſcination aſcribed to ſnakes.

The black-ſnake is a ſerpent of much more activity than the rattle-ſnake. The latter, as I have already ſaid*, ſeldom, if ever, climbs up trees. But the former will ſometimes aſcend the loftieſt trees, in purſuit of the object of his appetite. The rattle-ſnake, it has been juſt obſerved, ſubſiſts principally upon the large frog, which frequents the waters of our country. He has, therefore, but little occaſion for activity. But the black-ſnake, feeding more upon birds, ſtands more in need of activity. He frequently glides up the trees of the foreſt, &c. and, commonly in the abſence of the mother, devours either her eggs or her young ones. The difficulty of obtaining his prey upon the tree is ſometimes very conſiderable, as will appear from a fact which will be related immediately. Now, if this ſerpent is gifted with the faculty of faſcinating, why is he not content to continue at the bottom of the tree, and bring down his object? And if he can employ this machinery of faſcination at his pleaſure, how comes it, that he ſo ſeldom ſucceeds in capturing old birds? For it is a fact that when birds are found in his ſtomach, they are principally young birds.

I have ſaid, that the black-ſnake ſometimes finds great difficulty in obtaining his prey upon the tree. In ſupport of this aſſertion I could adduce many facts. But my memoir has already exceeded the limits which I originally preſcribed to it. I ſhall content myſelf, therefore, with relating a ſolitary fact, which ſtrikingly illuſtrates my poſition.

A black-ſnake was ſeen climbing up a tree, evidently with the view of procuring the young birds in the neſt of a baltimore-bird. This bird, it has been already obſerved, ſuſpends its neſt at the extremity of the branch of a tree. The branch to which the bird, of which I am ſpeaking, had affixed its neſt, being very ſlender, the ſerpent found it impoſſible to come at the neſt by crawling along it: he, therefore, took the advantage of another branch, which hung above the neſt, and twiſting a ſmall portion of his tail around it, he was enabled, by ſtretching the remainder of his body, to reach the neſt, into which he inſinuated his head, and thus glutted his appetite with the young birds.

The importance of this fact, in the inveſtigation of the ſubject of my memoir, appears to me to be great. An acts and ob- ervations re- pecting the ower of faſci- ation aſcribed o ſnakes.

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* See page 106.

American foreſt is not the ſilent reſidence of a few birds. During the greater part of the ſpring and ſummer months, our woods are alive with the numerous ſpecies of reſident and viſitant birds. At theſe times, if the black-ſnake poſſeſſes the faculty of faſcinating, it cannot be a difficult thing for him to procure his food. Yet, in the inſtance which I have juſt related, we have ſeen this reptile climbing up a tree, and there obliged to exert all his ingenuity to obtain his prey.

I cannot well conclude this memoir without obſerving, that in the inveſtigation of the ſubject which it involves, I have experienced much pleaſure. For to the cultivators of ſcience, the diſcovery of truth muſt, at all times, be a ſource of pleaſure. This pleaſure will even riſe to ſomething like happineſs, when, in addition to the diſcovery of truth, we are enabled to draw aſide the veil, which, for ages, has curtained ſuperſtition and credulity. Under the influence of various ſpecies of ſuperſtition, we fall from our dignity, and are often rendered unhappy. It ſhould be one of the principal objects of ſcience to rear and prop the dignity of the mind, and to ſmooth its way to comforts, and to happineſs. The ills and the infirmities of our earthly ſtate of being are numerous enough. It is folly, if not vice, to increaſe them. He who ſeriouſly believes, that an hideous reptile is gifted, from the ſacred ſource of univerſal life and good, with the power of faſcinating birds, ſquirrels, and other animals, will hardly ſtop here. He may, and probably will, believe much more. He will not, perhaps, think himſelf entirely exempted from this wonderful influence. He may ſuppoſe, that the property belongs to other beings, beſides the ſerpents; and he will, perhaps, imagine that it forms a part of a more extenſive plan, the effects of which, he will aſſert, are prominent, and unequivocal, though its ways, he will confeſs, are incomprehenſible to mortal minds.

Hiſtoria naturalis non bene digeſta abit in fabulam; præjudicia vero et nimia credulitas Veritatem, etſi cominus ſatis cognitam, longiſſime aliquando propellunt.

JACOBUS THEODORUS KLEIN.

Some

XV.

Some Account of an Egyptian Lock of very high Antiquity. Indicated from DENON'S *Travels, by a Correſpondent. With Obſervations by* W. N.

To Mr. NICHOLSON.

SIR,

A FEW weeks ago I ſaw, with particular pleaſure, a model, or wooden lock, made from the deſcription in Denon's travels, which was exhibited in the lectures at the Royal Inſtitution. I beg leave to propoſe the inſertion of the ſame in your excellent Work, and ſhould hope for your opinion as to its ſecurity.

Letter concerning a lock ſhewn at the Royal Inſtitution.

I am, Sir,
Your obliged reader,
P. Q.

New Broad Street,
May 12, 1804.

I HAVE given engravings of this lock, copied from Denon's book. *Plate* VII. ſhews the lock as applied to a door, and its developement is made in *Plate* VIII. The paſſage in Denon (tranſlated) is as follows:

Deſcription of the lock,

"No. 2, 3, 4, and 6. The Egyptian lock. It ſecures the gates of towns, of houſes, and the apertures of the ſmalleſt articles of furniture or uſe. I have placed it among the antiquities, becauſe it is the ſame as was in uſe four thouſand years ago. I found one ſculptured among the baſ-reliefs which decorate the great temple of Karnack. It is ſimple in conception, eaſy of execution, no leſs ſure than any other lock, and deſerves to be applied on all our rural occaſions. Fig. 2 is the key, which is capable of thouſands of different combinations. In Fig. 3 the lock is ſhewn cloſed, ſeen as to its interior; the key being in the act of lifting up certain pins which had fallen into holes in the bolt, and kept it in its place. In Fig. 1 the bolt is drawn back, and the lock conſequently open." *See plate 139 of the ſaid work.*

ſculptured on the great temple of Karnack.

Eton, in his Survey of the Turkiſh Empire, mentions this lock as follows:

"Nothing can be more clumſy than the door-locks in Turkey, but their mechaniſm to prevent picking is admirable. It

Eton's account of the ſame lock as of univerſal uſe in Turkey, &c.

It is a curious thing to ſee wooden locks upon iron doors, particularly in Aſia, and on their caravanſaries and other great buildings, as well as on houſe doors. The key goes into the back part of the bolt, and is compoſed of a ſquare ſtick with five or ſix iron or wooden pins about half an inch long, towards the end of it, placed at irregular diſtances, and anſwering to holes in the upper part of the bolt, which is pierced with a ſquare hole to receive the key. The key being put in as far as it will go, is then lifted up, and the pins entering correſponding holes raiſe other pins, which had dropt into theſe holes from the part of the lock immediately above, and which have heads to prevent them falling lower than is neceſſary. The bolt being thus freed from the upper pins, is drawn back by means of the key, the key is then lowered, and may be drawn out of the bolt: to lock it again the bolt is only puſhed in, and the upper pins fall into the holes of their own weight. This idea might be improved on, but the Turks never think of improving."

Obſervations.

Simpleſt ſtructure of this lock is a kind of latch.

The preſent lock.

Probable improvement.

THE ſimplicity and other advantages of this lock or bolt, are too obvious to require much remark; for which reaſon I ſhall confine my preſent obſervations to its degree of ſecurity or inviolability. I think we may contemplate it in three ſeveral ſtages of perfection or improvement. 1. If it be conſtructed with one pin of conſiderable ſize to fall into the bolt, and the finger be ſuppoſed to be introduced for the purpoſe of raiſing it and ſetting the bolt at liberty, we ſhall have a faſtening of nearly the ſame effect as the common latch. 2. Or if, inſtead of one falling pin, there be many, and an inſtrument be uſed to lift them, we ſhall have the bolt before us: and this, as far as we are informed, is the preſent ſtate of the invention, though of ſo long ſtanding. 3. Or thirdly, in caſe the preſent bolt ſhould, on examination, be found to admit of being opened without extreme difficulty, it will become a queſtion whether the principles of its ſtructure can be ſo applied as to render it abſolutely ſafe. This laſt queſtion requires that we ſhould firſt examine the ancient bolt a little more cloſely.

T[illegible]ck can[illegible] picked ;

We may admit that the ancient lock, with many pins falling independently of each other, cannot be picked or opened without its key: and therefore we muſt aſk whether the key can be made from an examination of the lock alone? In anſwer to this it may be noticed, that ſince all the pins in the key muſt be of equal height, the ſecret will conſiſt in their relative diſtances and poſitions on the face of the key; and that theſe diſtances and poſitions can be eaſily known by introducing a ſtick, or key without pins, into the hollow of the bolt, and taking an impreſſion (by means of a facing of ſoft clay) of the holes intended to receive the pins. After this a key may be made without difficulty; ſo that we arrive at our concluſion, that though this ingenious piece of mechaniſm cannot be violated without its key, yet it is eaſy to conſtruct a key for that purpoſe.

but its key may be eaſily made from the lock.

Improvement by which it is rendered ſafe.

We now come to the enquiry after that application of its principles which may render it abſolutely ſafe. This is certainly poſſible; but not without conſiderably diminiſhing its ſimplicity. Beſides ſeveral others, the following may be propoſed: Let the dropping pin have an enlarged part, and a tail of wire above and below. Let the lower tail fall into its hole in the bolt, while the enlarged part falls into a ſocket made for its reception. Under theſe circumſtances the bolt becomes faſt: But when by raiſing the pin the enlarged part is clear of its ſocket, the bolt becomes free, and the lower tail is prevented from ſtopping it by a groove left for its reception. The bolt muſt have a covering piece of board, having a hole of the ſize of the enlarged part of the pin, directly above the ſocket into which it falls, and a groove for its upper tail; the interval between the covering-piece and the bolt itſelf being equal to the height of the enlarged part of the pin. By this means, when the pin is puſhed up, juſt out of its ſocket, the bolt will move freely; but if it be puſhed the leaſt quantity farther, the enlarged part will enter the hole in the covering-board, and ſet it faſt, as if it were in the ſocket; ſo that a very preciſe diſtance of elevation will be requiſite. Laſtly, the lower pin may be ſhorter or longer at pleaſure. Now, if there be a number of theſe pins ſo placed and adjuſted as to fall into their reſpective ſockets at the poſition of the bolt when ſhut; if their lower tails be of different lengths, and a key be made to correſpond with them, and lift them all to the proper height at once; the combination will be ſuch as cannot be made out by any impreſſion or tentative proceſs upon the lock itſelf. For the evidence of a due length of any one of the lifting pieces of the key, will conſiſt in the actual opening of the lock; and this cannot be had unleſs the due length of all the pins be obtained at once; againſt which the probability will be as the number of permutations of the pins, multiplied into the number of poſſible lengths of pins practically differing from each other in effect. Thus if the pins were ſix, the permutations would be 1×2×3×4×5×6=720; and if the length of a whole pin were one quarter of an inch, and a ſenſible difference would in practice ariſe from making the pins one-ſixtieth of an inch longer or ſhorter, there would be 15 poſſible lengths for every pin. Whence 720×15=10800, the number of chances againſt the diſcovery.

W. N.

XVI.

On the Cauſe of the different Colours of the Triple Sets of Platina, and on the Exiſtence of a new Metallic Subſtance in that Metal. *By* Collet-Descotils.* *Preſented to the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute of France.*

Precipitation of the ſolution of platina.

ALL chemiſts know that crude platina is eaſily ſoluble only in nitro-muriatic acid, that the ſolution is decompoſable by muriate of ammonia and other ſalts with alkaline baſes; and that the reſult of their former decompoſition is a triple compound, conſiſting of oxid of platina, muriatic acid, and ammonia, or the alkali employed. The colour of this precipitate varies from a light yellow to a dark brown. It is ſometimes alſo greeniſh. The latter is the caſe if the ſolution of platina is precipitated by a ſalt with baſe of ſoda.

Remarks.

Before I ſay any thing further concerning the cauſes which influence the colour of this precipitate, I ſhall point out ſome phenomena which characteriſe the ſolution of the metal itſelf.

Foreign admixtures in crude platina.

The grains of platina of commerce always contain more or leſs of foreign mixtures from which it ſhould be previouſly freed as much as poſſible. The foreign bodies met with are moſt frequently minute ſtones, on which the acid employed for diſſolving the metal, has little or no action, and two ſorts of ferruginous

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* Gehlen's Chemical Journal, Vol. II. Part I. p. 73.

nous ſand, one of which is obedient to the magnet, and another which is not attracted by the magnet, and which is only partially acted on by acids. I ſhall ſay no more in this place concerning theſe bodies, but that the firſt contains titanium and the ſecond chromic acid, in conſiderable quantity.

The ferruginous ſands contain titanium and chromic acid.

The beſt method to free platina of commerce from theſe admixtures, is that recommended by Prouſt, which conſiſts in ſpreading out the platina on a ſheet of paper, and carefully blowing away the lighter parts by means of a pair of bellows.

Mechanical purification by bellows.

Platina thus purified I introduced into a porcelain retort, to which a glaſs receiver had been fitted, previouſly filled one-third full of water. After having placed the retort in a reverberatory furnace, I raiſed the fire gradually, and increaſed the heat to the utmoſt I could produce, which was kept up for two hours. Nothing particular attended this proceſs, except that the water with which the receiver had been partly filled, acquired a greeniſh hue towards the end of the proceſs. On the roof of the retort, a fine blue powder was ſublimed, of which I ſhall ſay no more at preſent than that it was ſoluble in water. The water in the receiver, after having been ſuffered to ſtand for a few days, had acquired a beautiful blue colour, which ſurpaſſed the colour of the beſt ultramarine.

Crude platina by violent diſtillation affords a blue ſublimate ſoluble in water.

It was impoſſible to get the platina out of the retort. On breaking the diſtillatory veſſel, the metal was found aglutinated, the upper ſurface of the maſs had a ruſty appearance, the middle was leſs diſcoloured, and the lower part had ſuffered no perceptible change.

The platina grains were agglutinated.

In order to examine the colouring matter which tinged the water of the receiver, I dropt into it a ſolution of an alcali; this produced inſtantly a blue precipitate. Sulphuric and muriatic acids, when mingled with this fluid, occaſioned no change. Nitric and oxiginized muriatic acids changed this blue fluid, firſt to a lilac, but it ſoon loſt this colour, and the whole became limpid. Water holding in ſolution ſulphuretted hydrogen gas, occaſioned no precipitate; but hidro-ſulphuret of ammonia threw down a grey precipitate, which became blue by the affuſion of acids, and then was rendered ſoluble again in hidro-ſulphuret of ammonia.

The aqueous ſolution of the blue matter afforded a precipitate by alcali. Habitudes with acids, &c.

A ſmall quantity of the blue precipitate collected from the roof of the retort, when urged with the blow-pipe in contact with borax, imparted no colour to this ſalt. When heated *per ſe*, it diſappeared completely.

The blue ſublimate diſappeared before the blow-pipe: it did not colour borax.

Having

When platina is diſſolved in n. m. acid, a black powder is ſeparated.

Having ſo far proceeded, I made a ſolution of platina in nitro-muriatic acid (the platina had been freed from iron as much as poſſible by muriatic acid.) During this ſolution, a glittering black powder became ſeparated, as is always the caſe when platina of commerce is diſſolved in nitro-muriatic acid. If the operator be careful in collecting this powder as faſt as it is ſeparated, the quantity which may be collected, amounts to about 0,03 of the platina employed. But if this powder be not removed as faſt as it is depoſited in the ſolution, part of it becomes again acted upon, and a much leſs quantity is obtained.

Muriate of pl. was decompoſed by mur. of ammonia.

Yellow precipitate.

The decanted fluid being concentrated, and more muriate of amm. added, a *dark red* precipitate was obtained.

The fluid a ſecond time decanted gave by mur. amm. a dark brown precip.

The muriate of platina obtained, after having been ſuffered to repoſe and being decanted, I decompoſed by adding by a ſaturated ſolution of muriate of ammonia; the precipitate was ſeparated by decanting the ſupernatant fluid, and repeatedly waſhed till the water which paſſed, did not become green by the addition of pruſſiate of potaſh. The precipitate obtained was of *a yellow colour*. The decanted fluid from which the precipitate had been ſeparated, and the firſt quantity of water employed for its ablution, after having been mingled together and concentrated by evaporation to one third of its bulk, were again mixed with a ſolution of muriate of ammonia; the precipitate now obtained was of a *dark red colour*. On treating the fluid ſeparated from it, as before, the precipitate which did fall down was of a very *dark brown*. All theſe different coloured precipitates were carefully waſhed till they contained no veſtige of copper or iron.

The colour of the precip. of platina is darker the more of the black powder in the ſolution.

I have remarked that if the ſolution of platina be ſlowly prepared, that is to ſay, if the platina be introduced into the nitro-muriatic acid, in ſmall quantities at a time, and the ſubſequent ſolution of each quantity be reſpectively ſeparated, and ſeparately be decompoſed by muriate of ammonia, the colour of the precipitate or triple ſalt obtained, is darker in proportion to the quantity of the black powder which was contained in the ſolution.

The black powder is (difficultly) ſoluble in nitr. m. acid; and decomp. by mur. of am.

This black powder which is depoſited during the ſolution of platina of commerce* in nitro-muriatic acid, is ſoluble (though difficultly) in nitro-muriatic acid, compoſed of much nitric, and little muriatic acid; its ſolution is alſo decompoſable by muriate of ammonia, and the colour of the precipitate is more or leſs intenſe, according to the quantity of powder contained in the ſolution.

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* This black powder is likewiſe ſeparated during the ſolution of *malleable platina* in nitro-muriatic acid. F. A.

From what has been ſo far ſtated, it appears, that this black powder is the cauſe of the different colours which the different precipitates or triple ſalts of platina exhibit, under different circumſtances.

The black powder is the cauſe of colour in the precipitates.

In order to learn the nature of this ſubſtance, I ſhall detail the experiments which were undertaken for that purpoſe. The precipitates or ſalts I made uſe of were, the triple ammoniacal muriate of platina, and the triple muriate of platina and ſoda; the former ſalt I preferred on account of its eaſy decompoſibility, and the latter on account of being very ſoluble.

Enquiry into the nature of this ſubſtance.

Experiments on the triple ammoniacal Muriate of Platina.

Equal quantities of the before-obtained yellow and dark red precipitate, being ſeparately diſſolved in equal quantites of water, the firſt ſalt furniſhed a ſolution of a gold yellow colour, whereas that of the latter was orange red. On adding to the latter ſolution a minute quantity of green ſulphate of iron, or ſulphureous acid, it became inſtantly of a gold yellow colour; the ſame effect was produced, though ſlowly, by the addition of alcohol.

Aqueous ſolutions of the yellow and dark red precipitates; the firſt gold-yellow, the ſecond orange red.

The latter was rendered yellow by a ſm. portion of gr. ſulph. of iron, or by alcohol.

It was natural to ſuppoſe that the colour of the red ſalt might be owing to the higher degree of oxidizement of the diſſolved platina. In order to convince myſelf of this conjecture, I attempted to transfer oxigen to the yellow ſalt, by means of nitric and oxiginized muriatic acids. This however failed; the colour of the ſalt remained yellow as before. On repeating the application of nitric and oxiginized muriatic acids alternately, the reſult was only a very pale red coloured precipitate.

The red colour was not owing to greater oxidizement.

Equal parts of the yellow and red precipitate, deſiccated at equal temperatures, and under the ſame circumſtances, when decompoſed by heat, yield alſo unequal quantities of fixed reſidue. That of the firſt, amounted to 0,44 of the weight of the ſalt employed, and that of the latter was 0,4 and 5.

The yellow and red precipitates do not leave equal reſidues, when decompoſed by heat.

If yellow precipitate be reduced by heat, the platina obtained is *uncommonly ſoluble* in a comparatively ſmall quantity of nitro-muriatic acid, and the ſolution yield a yellow precipitate with muriate of ammonia.

The yellow precipitate if reduced, affords very ſoluble platina; which again affords a yellow precip.

If

The red precipitate affords a leſs ſoluble metal which leaves a black powder, and affords a red precip.

If the red precipitate be reduced, the metal obtained is of a different nature from the former. It is far more inſoluble in nitro-muriatic acid, and whatever quantity of acid may be applied, there remains conſtantly a quantity of black powder, which is abſolutely inſoluble in the acid. This ſolution, on being decompoſed by muriate of ammonia, yields a red precipitate.

This precipitate treated with oxiginized muriate of potaſh with heat.

To learn the nature of this precipitate, I reduced a quantity of it, introduced it into a porcelain tube, connected with a ſmall retort, containing hyper-oxiginized muriate of potaſh, and applied heat to the retort; after having firſt adapted to the other extremity of a receiver containing a little water. On increaſing the heat ſo as to decompoſe the hyper-oxiginized muriate, the tube became lined with a blue powder, which was alſo obſerved in the empty part of the receiver.

Blue powder obtained;

After all the ſalt in the retort had been decompoſed, I collected the blue ſublimed powder. The minute quantity of it however did not permit me to ſubmit it to many experiments. It was eaſily ſoluble in nitro-muriatic acid. Its nature will become more obvious hereafter.

—ſoluble in nitro-muriatic acid.

The red colour ariſes from a peculiar metal.

From what has been ſo far ſtated, it appears to follow that the red colour of the triple precipitate of platina, obtained by muriate of ammonia, or other ſalts with alkaline baſes, is owing to the preſence of a peculiar metal contained in the platina, which has hitherto been conſidered as ſimple.

Examination of the triple Muriate of Platina and Soda.

Triple muriate of platina and ſoda.

This triple compound is very little known.[*] It may eaſily be obtained by pouring into a ſolution of platina, a ſalt with baſe of ſoda. It is very ſoluble in water, and even in alcohol. The ſolutions are capable of cryſtallizing in long priſms, on three-ſided tables, of a yellowiſh red colour. It is decompoſable by muriate of ammonia; the precipitate is a muriate of platina and ammonia. It is likewiſe decompoſed by a ſolution of ſoda; on adding this alcali in exceſs, the formed precipitate becomes again diſſolved.

Reducible on charcoal, by the blow-pipe.

Muriate of platina and ſoda is reducible upon charcoal before the blow-pipe. The reduced metal poſſeſſed a conſiderable luſtre.

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[*] Muſſin-Puſchkin has pointed out ſome of its properties in Crell's Annales, 1800, Vol. I. p. 93. of which a ſhort abſtract is to be found in the Annales de Chimie. p. 277.

If cryſtallized muriate of platina and ſoda, free from all adhering or exceſs of acid, be expoſed for ſome time to the air, its beautiful red colour becomes changed into a greeniſh hue. If the ſalt in that ſtate be diſſolved in water, and oxiginized muriate of lime be added to it, a dark brown precipitate falls, which after having been waſhed and dried, is ſoluble in muriatic acid, with which it forms a *beautiful blue ſolution.* This colour becomes again deſtroyed by the admixture of alcohol, and re-appears by the addition of oxiginized muriate of lime.

Its cryſtals from a beautiful red, become green by expoſure to the atmoſphere.

Its ſolution precip. by ox. muriate of lime.

This precipitate is ſomewhat ſoluble in water; it is reducible when fuſed with borax, without imparting to the latter any colour. The reduced metal is very porous: it appeared not to be ſoluble in any of the acids.

Precipitate ſomewhat ſoluble; reducible into a porous metal inſoluble in acids.

If the ſolution of muriate of platina and ſoda, contain an exceſs of acid, it then is not diſturbed by letting fall into it oxiginized muriate of lime; but on evaporation the mixture acquires a fine *green colour.*

No precipitate if acid be in exceſs.

Examination of the Yellow and Red triple Muriate of Platina.

If we add to the red triple muriate of platina, a ſolution of carbonate of ſoda, till it becomes completely diſſolved the ſolution is of a yellow colour. On expoſing it for ſome time to the contact of air, a green ſubſtance becomes depoſited.

The red triple muriate becomes yellow by carbonate of ſoda; —and depoſits a green precip. by expoſ. to the air.

The yellow triple muriate of platina, treated in a like manner with carbonate of ſoda, yields a ſolution of a dark yellow, or orange colour, which ſuffers no change whatever on expoſure to air.

The yellow thus treated becomes darker, and does not afterwards change.

The ſeparation of the green ſubſtance from the red triple muriate, is much accelerated by the admixture of oxiginized muriatic acid. It ſeems as if the ſeparation of this ſubſtance is owing to the action of oxigen; for no other acid is capable of producing it; at leaſt the precipitate which they produce, is a mere oxid of platina.

The green depoſit from the red triple muriate is from oxidizement.

The green ſubſtance may likewiſe be inſtantly obtained by evaporating the ſolution by heat.

It is inſtantly had by evaporation.

If the ſolution of the yellow triple muriate be heated and evaporated, a very minute quantity of the green ſubſtance is indeed, ſometimes depoſited, and the remaining ſolution then acquires a more beautiful yellow colour. On continuing the evaporation of the yellow ſolution, a precipitate is depoſited, which

The yellow triple muriate may afford a minute portion of green precip. by evap.—but this is an impurity.

Its precip. is yellow.

which is not green, but *yellow*. This precipitate on being again diſſolved in muriatic acid, and ſaturated with carbonate of ſoda, yields no farther green ſubſtance.

The green precipitate was reduced (by heat with borax) to a white brittle metal; difficultly acted on by nitro-m. acid;

I mingled a quantity of this green ſubſtance with vitrified borate of ſoda, and expoſed the mixture for twenty-five minutes in a double crucible to the moſt intenſe heat I could produce; the reſult was a white brittle metal, which was difficultly acted upon by nitro-muriatic acid. The weak ſolution which had been obtained by this acid, had a violet colour; it yielded, on being evaporated, a dark green reſidue, which was ſoluble in muriatic acid, with which it formed a green fluid.

more ſo when powdered.

On pulverizing another quantity of this white brittle metal, and then expoſing it to the action of nitro-muriatic acid, the ſolution was effected more eaſily, it was now more concentrated, and of a reddiſh-yellow colour. Muriate of ammonia let fall into it, threw down a little browniſh-red precipitate, a proof that this metal ſtill contained platina.

The ſolution of the green ſubſtance became yellowiſh by ſulphureous acid or green muriate of iron.

A quantity of the before-mentioned green ſubſtance being diſſolved in muriatic acid, acquired a yellowiſh colour on being mingled with ſulphureous acid, or with a ſolution of green muriate of iron. Oxigenized muriate of lime reſtored the original green colour.

Ox. mur. of lime reſtored its green.

The green ſubſtance gave a blue ſublimate, and left metal.

A third part of the green ſubſtance, on being expoſed to a violent heat in a porcelain retort, yielded a blackiſh-blue ſublimate: the unſublimed part was a metallic ſubſtance, very difficultly ſoluble in nitro-muriatic acid.

A portion of the ſame ſubſtance heated with nitre till the acid was decompoſed. The alkali was diſſolved in water, and not changed by acids. The reſidue was not acted on by acids. Hence chrome and molybdena were abſent.

A fourth portion of the green ſubſtance was urged, in a crucible head, with nitrate of potaſh. After the decompoſition of the nitrate had been completed, the maſs was diffuſed through water. The alkaline ſolution was colourleſs, and acids produced no change of colour in it. The reſidue, after repeated ablutions, was hardly acted upon by acids, the nitro-muriatic acid not excepted. This experiment precludes the preſence of chrome and molybdena.

Separation of the platina from the triple muriate by alcohol.

It is poſſible to ſeparate nearly all the platina which is contained in the red triple muriate, by the mere admixture of alcohol, and the ſubſequent addition of dry potaſh or ſoda; for doing this much heat is evolved, and the reduction of platina takes place almoſt inſtantly. The ſame may be effected by means of carbonate of potaſh or ſoda; but in that caſe the ſolution muſt be highly concentrated. The reduction of the platina then takes place even without the application of heat, but only after the ſpace of ſome days.

The ſupernatant fluid of the reduced platina, when heated, acquires a lilac colour, which becomes blue on long expoſure to air, depoſiting at laſt a greeniſh ſubſtance, reſembling that hitherto treated of. Oxigenized muriatic acid favours the ſeparation of this ſubſtance.

The ſupernatant fluid affords the blue ſubſtance.

Sulphuretted hidrogen may alſo be employed for ſeparating the platina from the red triple muriate; the platina becomes ſeparated in the form of a brown powder; the other metallic ſubſtance remains undiſturbed in the ſolution. It may, however, almoſt totally be precipitated by liquid ammonia. The precipitate obtained is brown. When fuſed with potaſh in a ſilver crucible, the maſs acquires a green colour: On pouring muriatic acid on it, no complete ſolution could be effected; there always remained a powdery ſediment which reſiſted likewiſe the action of nitro-muriatic acid. Carbonate of potaſh ſeparated from this ſolution a ſmall quantity of iron. The clear fluid from which the iron had been ſeparated, remained perfectly tranſparent when heated; but it acquired a bluiſh hue, which increaſed on concentration, and after the exſiccation of the ſalt. On adding a little nitric acid to the ſalt, the blue colour became changed into a dark red.

Sulphuretted hidrogen alſo ſeparates the platina.

CONCLUSION.

I now flatter myſelf with having proved, by the above experiments, that the red coloured triple ſalts of platina owe their colour to the preſence of a peculiar metal, oxidized to a certain degree.

Concluſion.

The red colour is owing to a peculiar oxide,

That this metal is nearly wholly inſoluble in acids; that it becomes ſoluble in combination with platina; that when oxidized, it appears in the form of a blue oxide inclining to green; that its oxides, when combined with platina, are ſoluble in alcalies; that its acid ſolutions are not decompoſable by ſulphuretted hidrogen; that it imparts no colour to borax; that its oxides are reducible and volatilizable by heat, which volatilization becomes favoured by a ſtream of oxigen gas; and, laſtly, that oxigen gas, aſſiſted by heat, is capable of oxidizing this metal, and of volatilizing it in the form of a blue oxide.

nearly inſoluble in acids, unleſs combined with platina, &c.

Theſe

The new metal not yet named. Theſe properties do not characterize any of the known metals, and I am therefore authorized to conſider it as a new one, to which I ſhall give a name when I have explored its nature more fully.

XVII.

Deſcription of an Apparatus for filtering Water. By Meſſ. HARMAN *and* DEARN, *of Redriff.*

Apparatus for filtering water. THE waters which run near or upon the ſurface of the earth, are uſually contaminated by the remains of animal and vegetable ſubſtances in their progreſs towards entire decompoſition, as well as by the minute powder of earthy or mineral bodies, which render it turbid and leſs fit for the purpoſes of domeſtic life. Spring or pump waters, by a natural filtration through the ſandy ſtrata of the ground, are moſtly cleared from theſe mechanical admixtures; but in many places, as is the caſe with thoſe of our metropolis, they are rendered impure, or, as it is called, hard, by an actual ſolution of ſulphate of lime or plaſter of Paris, which prevents their lathering with ſoap, and probably renders them leſs wholeſome; beſides which, they uſually carry a portion of the drainage water in great towns, which renders them offenſive at certain ſeaſons, and at all times leſs worthy of confidence. For theſe and numerous other reaſons, it has always been conſidered as a deſirable object to clear waters, by filtration, from thoſe impurities which render them leſs limpid, and a variety of apparatus have been offered to the public for that purpoſe.

In all theſe the proceſs of nature has been imitated; namely, by cauſing the water to percolate either through ſand or a ſand-ſtone; the latter of which, though coſtly, ſeems at preſent to be almoſt the only method in uſe among us.

The contrivers of the ſimple and cheap apparatus delineated in Plate VI. are Meſſ. Harman and Dearn, potters at Redriff, who remark, that the filtering-ſtone is not only expenſive and liable to be clogged up and ſpoiled by the bodies depoſited in its pores from the water, but that, as theſe bodies are actually in the progreſs to decay and decompoſition, they are in ſome caſes found actually to change the flavour and affect the purity of the fluid they are made uſe of to ameliorate. In conſequence of which, they have been induced to apply their art to the ſimpleſt method of affording an apparatus for the filtering proceſs, which ſhall not be liable to theſe objections.

Apparatus for filtering water.

Plate VI. Fig. 1, repreſents the whole apparatus. Fig. 2 ſhews a ſhaded ſection. A is a veſſel of ſtone-ware perforated with holes, *m*, at bottom, upon which coarſe gravel, *h*, is laid, and upon that a ſtratum of fine gravel, and laſtly fine ſand, *g*. Or otherwiſe, the bottom may be covered with a coarſe cloth, which will render the graduated fineneſs of the gravel and ſand leſs neceſſary. Upon the top of the ſand is laid a perforated and loaded board or plate of earthen ware, to prevent the ſand from being diſturbed when the water is poured in. B is a lower veſſel, into which the filtered water from A drops, together with any ſand that may eſcape from above. The clear water flows out through the neck *a c* into the veſſel D for uſe.

The ſtructure, uſes, and effects of this apparatus are ſo obvious, that it is needleſs to enlarge upon them. The fineneſs and depth of the ſiliceous ſand will regulate the perfection and expedition of the proceſs; and the requiſite cleanneſs and delicacy of the veſſels and ſand may be inſured by changing the latter from time to time; for example, once in a fortnight or three weeks.

W. N.

XVIII.

Examination of a Stone containing Potaſh. By FREDERIC ACCUM, *Teacher of Practical Chemiſtry, Pharmacy, and Mineralogy.*

To Mr. NICHOLSON.

SIR,

Introductory letter.

SINCE Klaproth has detected potaſh in the lepidolite, leucite, and ſonorous porphyry, chemiſts have ſought for this alkali in other minerals, and their enquiries have not been diſappointed: Tromſdorff has found it lately in the augite, and no doubt this alkali will be met with in many other minerals in which it was not expected. Being called upon by the company of potters of Staffordſhire to examine a variety of ſtones employed in the manufactory of earthen-ware, a particularly exact analyſis was demanded of a ſtone, labelled *grey Corniſh**, in which I detected this alkali. I ſhall not detain you with a circumſtantial detail of my reiterated experiments, which were undertaken with a view to learn the nature of the ſtone under conſideration, which is ſought for by the potters with avidity; I ſhall merely confine myſelf to point out its characters, as well as that examination which may ſerve to eſtabliſh the credit of my aſſertion.

I am, Sir, with reſpect,
Your moſt obedient humble ſervant,
FREDERICK ACCUM.

Old Compton Street, Soho.

Phyſical Characters of a Siliceous Stone containing Potaſh.

Phyſical characters of a ſtone containing potaſh.

THIS ſtone is found in amorphous maſſes, forming irregular ſtrata, under the ſurface of a blue clay. Its colour, when newly taken from the bed, is a greeniſh-grey interſperſed with black ſpots; but when left expoſed to the air, it acquires an aſh-grey colour. It is not very hard; its powder is white. When rubbed, it exhales a faint argillaceous odour. Its ſubſtance is coarſe or uneven, having many ſmall, ſharp, abrupt, irregular elevations and irregularities. Its fracture is very irregular. It may be eaſily ſcratched with a knife. It ſcintilates with ſteel. It is abſolutely opaque in ſmall fragments. Urged before the blow-pipe, it froths and melts into a white enamel. Treated with borax in a ſimilar way, it forms a reddiſh bead.

Its ſpecific gravity is 2,465.

ANALYSIS.

Analyſis of a ſtone containing potaſh.

Experiment I.—One hundred grains of the finely le[illegible]d ſtone, after having been previouſly ignited†, were mi[illegible]ith a ſolution of potaſh containing 400 grains of alkali, th[illegible]xture was evaporated in a ſilver capſule to dryneſs, transferred

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* I am not permitted to ſtate the exact place in Cornwall where this ſtone is found.

† 100 parts loſt, during ignition, 6 parts.

Analyſis of a ſtone containing potaſh.

into a crucible of ſilver, and fuſed for half an hour. The fuſed maſs had rather a paſty appearance, and could not be rendered perfectly fluid.

Exp. II.—As ſoon as the crucible was nearly cold, it was removed from the furnace, its contents were ſoftened by water, and the affuſion of this fluid renewed from time to time, till all the fuſed maſs was detached from the crucible: about 18 times its quantity of water were expended for that purpoſe.

Exp. III.—Into the obtained alkaline imperfect ſolution of muriatic acid was gradually poured, and the whole evaporated to dryneſs.

Exp. IV.—The maſs was then tranſferred into a flaſk containing dilute muriatic acid, the whole was ſuffered to boil for a few minutes, and the inſoluble part ſeparated by the filtre. The ſiliceous earths thus obtained had a greyiſh appearance, but it acquired a white colour after having been again digeſted in muriatic acid, dried and ignited. It weighed 58 grains.

Exp. V.—The fluid from which this quantity of ſilex had been ſeparated, together with the muriatic acid employed for purifying it, and the water expended for ablution, were concentrated to about 20 cubic inches, and then mingled, boiling hot, with a ſolution of carbonate of ſoda; the precipitate depoſited was collected, and waſhed as uſual.

Exp. VI.—As ſoon as the precipitate was ſo dry that it could be removed from the filtre without loſing part of it, it was tranſferred into a ſolution of potaſh, and the mixture boiled for about half an hour. On ſuffering the alkaline ſolution to ſtand undiſturbed for twenty-four hours, a powder was depoſited, which, on being again boiled in a more concentrated ſolution of potaſh, remained unaltered. It was therefore collected, waſhed, dried, ignited, and put aſide for further examination.

Exp. VII.—Into the alkaline ſolution, freed from this powder, I now dropt muriatic acid, till the precipitate which firſt appeared again vaniſhed, and then decompoſed it by the addition of carbonate of ammonia. The precipitate obtained in this experiment, after repeated ablutions and ignition, amounted to 28 grains. Experiments not eſſential to be ſtated here, convinced me that it was alumine.

Analyſis of a ſtone containing potaſh.

Exp. VIII.—The precipitate of this experiment was diſſolved in nitro-muriatic acid, and then mingled with liquid ammonia. A brown precipitate became depoſited, which, after being dried, amounted to 0,25 grains. It was oxide of iron.

The fluid from which this iron had been ſeparated, yielded no other products, on being carefully examined, except two grains of ſilex which eſcaped ſeparation.

Thus far the ſtone had yielded ſiliceous earth, alumine and oxide of iron. I ſhall ſuppreſs thoſe enquiries which proved fruitleſs for learning its farther compoſition, and ſtate merely thoſe which were attended with ſucceſs for that purpoſe, and which were as follow:

Exp. IX.—One hundred grains of the ſtone reduced to an impalpable powder, were triturated with 600 grains of cryſtallized nitrate of barytes: the mixture fuſed till the flame of a piece of ignited wood did not become enlarged when held cloſe over the ſurface of the fuſing maſs.

Exp. X.—The melted maſs was ſoftened with water, and digeſted in muriatic acid; the fluid filtered, and the reſidue waſhed.

Exp. XI.—The liquid obtained in this proceſs I neutralized with carbonate of ammonia, and added the latter till no further cloudineſs enſued. After having ſeparated the precipitate, I evaporated the fluid to dryneſs: the obtained ſalt I transferred into a glaſs tube cloſed at one end, and committed it to ſublimation.

Exp. XII.—After the ſublimation had ceaſed (which was known by a metallic wire introduced into the tube during the proceſs, not becoming in the leaſt covered with a coat of ſalt), I broke the tube and ſeparated the ſalt. This being diſſolved in water and cryſtallized, yielded nine grains of muriate of ſoda.

Exp. XIII.—To decompoſe this ſalt, I rediſſolved it in water, and mingled the ſolution with nitrate of ſilver till no farther cloudineſs appeared: I then ſeparated the muriate of ſilver formed, and decompoſed the nitrate of potaſh by heat, which yielded 4,50 of potaſh.

The analyſis being thus completed, I learnt that 100 grains of this ſiliceous ſtone from Cornwall contain,

Analyſis of a ſtone containing potaſh.

Silex, - - -	60
Alumina, - - -	28
Oxide of iron, - -	0,25
Potaſh, - - - -	4,50
Water, - - -	6
	98,75
Loſs, -	1,25
	100

XIX.

Letter from A. Carlisle, *Eſq. on the Temperature of the Sea.*

To Mr. NICHOLSON.

Dear Sir, *Soho Square, May* 28, 1804.

THE following table was made by Mr. R. Perrins, ſurgeon on the Honourable E. I. Company's eſtabliſhment, during a voyage to Bombay in the year 1800. The temperatures in this table, were noted at my requeſt, from a deſire to determine whether fiſhes poſſeſs any other temperature than that of the water in which they live, the negative being aſſerted by Linnæus. As, however, this imperfect journal may aſſiſt in ſimilar reſearches, I beg leave to offer it for the uſe of your periodical work.

Table of the temperature of the ſea, &c.

I am, Sir,
Your much obliged ſervant,
ANTHONY CARLISLE.

Table of the temperature of the ſea, &c.

" *Regiſter of Atmoſpheric Temperatures, collated with thoſe of the Sea-Water taken on board the Honourable E. I. Company's extra Ship Skelton Caſtle.*"

1800.	Atmo-ſphere.	Sea-Water.	Lat. N.	Long. W.
Feb. 28	*	52°	42° 34′	13° 25′
March 2	*			
4	*	57	36 47	15
7	*	62	33 19	17 34
9	*	64	31 58	50
11	*	66	30 26	20 49
13	*	67	No obſerv.	59
15	71°	†68	26	23 24
16	72	69½	25 49	24
17	72	70	21 14	55
18	74	72	18 16	26 12
19	74	72	15 17	47
20	75	72	13 52	24 49
21	76	74	11 58	23 6
22	78	†76½	9 46	21 12
23	80	78	7 43	20 11
24	82	80	5 25	20 13
25	82	80	4 24	20 6
26	84	82	3 33	19 41
27	84	82	2 58	19 27
28	85	82	2 35	19 17
29	86	83	2 7	19 10
30	86	83	35	20 1
31	85	82	33 S.	19
April 1	84	81	2 27	20 41
2	83	80	4 44	22 31
3	82	80	7 12	23 47
4	83	80	9 50	24 50
5	82	79	12 30	25
6	80	76	15 17	25 59
7	78	76	18 13	26 5
8	78	76	20 41	26 3
9	80	78	22 22	56
10	80	78	23 28	27 20
11	80	78	24 23	37
12	78	76 Shark 88‡	24 48	32
13	76	74	26 29	22
14	74	72	27 50	24 14
15	72	70	28 42	35
16	72	70	31	45

* The atmoſpheric temperature was not ſet down during theſe days.

† Each trial upon the ſea-water was repeated three or four times, and the ſame reſults followed.

[*Table continued.*]

Table of the temperature of the ſea, &c.

1800.	Atmo-ſphere.	Sea-Water.	Lat. S.	Long. W.
April 17	70°	68°	33° 10′	23° 57′
18	70	66	34 20	22 46
19	70	66	35 51	19 38
20	68	66	36 2	18 35
21	68	66	36 3	15 11
22	68	64	35 56	11 22
23	68	64		6 47
24	66	64	36 47	3 15
25	64	62	36 34	41 Eaſt
26	64	62	39	3 40
27	62	58	37 25	6 20
28	61	58	53	7 40
29	60	58	45	8 40
30	60	58	36 30	12 11
May 1	60	58	7	16 13
2	60	58	30	18 13
3	60	60	34	10 16
4	60	60	37 22*	22 25
5	60	62	10	26 38
6	58	64	36 54	50 51
7	62	66	30	34 51
8	64	66	37	39 39
9	62	64	5	43 24
10	62	64	20	45 21
11	62	64	35 43	48 14
12	64	66	33 15	49 38
13	66	66	30 35	51 18
14	68	68	27 35	52 12
15	72	70	25 2	31
16	76	74	21 21	39
17	78	76	18 52	53 16
18	78	76	16 25	54 12
19	80	78	13 27	53 29
20	78	78	12 41	50 55
21	80	78	10 56	49 41
22	80	78	7 8	49 41
23	80	80	4 9	50 32
24	81½	80	1 46	52 27
25	84	81	13 N.	54 2
26	86	82	2 32	50 59
27	84	82	4 36	56 10
28	85	82	9 26	60 51
29	85	82	9 26	60 51
30	85	82	14 31	64 41
June 1	84	82	15 56	66 59
2	86	82	17 39	69 40
3	85½	84	18 57	72
4	Arrived in Bombay Harbour.			

XX.

Obſervations upon the Doctrine of Count Rumford reſpecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.*

The doctrine of Count Rumford on the non-conducting property of fluids deſerves examination.

COUNT RUMFORD has publiſhed ſeveral memoirs, by which he has endeavoured to prove that liquids and elaſtic fluids are not conductors of heat, and that they only tranſmit caloric by means of the contact with ſolid bodies, which is owing to the motion of their parts: as this property would make a difference between the ſtates of a ſubſtance much greater than there is occaſion to ſuppoſe in the explanation of the other phenomena; as, beſides, this celebrated philoſopher has fixed the attention on an object which had been neglected, and has drawn applications from it, beneficial in the arts, and in the uſes of life, I think it proper to offer ſome doubts on the principles which he has deduced from his obſervations: I ſhall, in the firſt place, examine whether the facts on which he relies, cannot admit of a natural explanation from the properties which I have already analyſed, or whether it will be neceſſary to have recourſe to particular properties. But I ſhall attend only to the conſiderations which may ſerve to elucidate this diſcuſſion, without introducing the details it would require, if I were to examine it more fully.

Detail of Count Rumford's experiments on heated fluids.

The experiments which the author made were performed with an apparatus, of which it will be proper to inſert a deſcription. "He employed a cylindrical glaſs jar of 4.7 inches in diameter, and 13.8 in height; he put a known quantity of water (about two pounds) into the jar, which was intended to form a cake of ice at the bottom of the veſſel. For this purpoſe, the jar with the water was put into a frigorific mixture of ſalt and ice, the action of which was not long in converting the water into a ſolid diſk adhering to the bottom and ſides of the jar; the jar was then removed, and plunged into a mixture of ice and water, to the level of the interior cake, which gave it the temperature of melting ice, or of the zero of the common thermometer. Then, after having covered the ſurface of the ice with a diſk of paper,† hot water was poured on it, as gently as poſſible, to about the quantity of 74 ounces; this water was about eight inches above the ſurface of the diſk."

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* From his Eſſai de Statique Chemique.

† Bibl. Brit.

Detail of Count Rumford's experiments on heated fluids.

"The paper was then removed very gently, and after having ſuffered the water to remain a certain number of minutes in contact with the ice, it was poured off, and the jar with the ice which it ſtill contained immediately weighed; its difference from the primitive weight eſtabliſhed the quantity of ice which had melted while the hot water remained above it."

Having obſerved that the motion occaſioned by pouring on the hot water produced an effect which was conſiderable, and foreign to the communication of heat, the author ſucceſſively deviſed ſeveral modes of diminiſhing it. "He introduced the hot water through a wooden tube, cloſed at the bottom and pierced laterally with ſeveral ſmall holes, through which the water iſſued upon a wooden diſk, alſo pierced like a ſieve, and floating on the water as it roſe in the veſſel. This diſk was removed as ſoon as the water was poured in, and the veſſel was covered with a wooden lid, in the centre of which was ſuſpended a thermometer; finally, by previouſly covering the ice with a ſtratum of cold water, about half an inch in thickneſs, in which the perforated wooden diſk floated, the author ſucceeded in greatly diminiſhing the irregularity of the reſults."

Beſides, theſe precautions, the author ſeparated from his reſults the quantity of ice, which liquefied at the firſt inſtant, and which exceeded that which melted in the ſucceeding ſpaces of time: in theſe different experiments, while that part of the cylinder which contained the ice was kept conſtantly at the temperature of melting ice, the upper part was left in contact with the ſurrounding air, or ſurrounded with a bad conducting ſubſtance, or plunged into the mixture of water and ice: the water poured on the ice received different temperatures. I make three diviſions of the reſults of all the experiments: 1ſt. Water which was only about four degrees above zero, melted a little more ice in the ſame ſpace of time than boiling water: 2d. When the upper part of the cylinder was wrapped in a bad conducting ſubſtance, the hot water melted more ice than when it was in contact with the air: 3d. When the upper part of the cylinder was plunged into the mixture of ice and water, more ice was melted than when it was left in contact with the atmoſphere at 61° Fahrenheit's thermometer.

Explanation. 1. More heat will be communicated *the greater the difference of temperature.* 2. Locomotion increaſes this effect in fluids, and ought to be ſeparately conſidered. 3. The locomotion does not follow the difference of temperature.

To explain theſe obſervations, the properties which we have recognized in liquid ſubſtances, and in elaſtic fluids, and from which we have inferred the changes which are effected in their different ſtates of combinations, muſt be applied to the phenomena obſerved by Rumford.

We have ſeen, 1ſt. that the liquid particles enter ſo much the more rapidly into combination as they were at a greater diſtance from ſaturation, becauſe then the force which ſolicits the ſaturation is greateſt; ſo that the effects which depend on the communication of the temperature muſt be very weak, when the differences between them are but ſmall.

2d. Locomotion, which ſerves to bring particles together which are at a greater diſtance from ſaturation, accelerates the effect of the mutual action by which its equilibrium is eſtabliſhed, ſo that it is neceſſary to ſeparate the effect which depends on this cauſe from that which is owing to immediate communication.

3d. Water and ſome other ſubſtances acquire a greater ſpecific levity on approaching the term of congelation; whence it follows that the locomotion produced by the variations of temperature in other circumſtances will be ſubject to modifications, which muſt be allowed for when water and the other liquids, which poſſeſs this property, approach the term of congela[illegible]

To apply theſe properties, we muſt alſo take into co[illegible]ation the direction in which the heat is communicated; for the combination of effects will be different accordingly as it is applied to the inferior or ſuperior part of a liquid.

Locomotion is greateſt *when the difference of temperature is ſmall.*

In order that a ready motion may be eſtabliſhed between the particles which are at the bottom of the veſſel, and thoſe at the ſurface, there muſt be but little difference between their temperature; the particles which are near the ice, and become expanded, will then raiſe themſelves above thoſe which have a temperature barely greater; but if the temperature ſhould cauſe a great difference between the ſpecific gravities, this motion will be much more confined, ſo that the ice will remain ſurrounded with water of its own temperature, or which is very little removed from it. It is evident, therefore, that that part of the effect which depends on the motion will be much leſs, when there is a great difference in the temperatures.

The effect of the heat by a fluid will be [illegible]

But when this diſtance in fact exiſts, the effect produced by the communication of the heat, independently of the motion, will greateſt where the difference of temperature is moſt favourable to both the cauſes of its communication.

C. Rumford's experiments require no new law to explain them.

will vary according to the manner in which the temperature is preſerved in the liquids. If the veſſel has a non-conducting covering, the heat will be retained, and a greater quantity will be communicated than if it had been allowed to paſs into ſurrounding bodies.—But when the difference of the temperature of the liquid is not conſiderable, as in the experiment in which water at 16° was employed, it is more advantageous to augment the effect owing to the tranſlation of the particles, by cooling all the cylinder, than to preſerve that which is owing to the ſimple communication of caloric. It appears to me that this explanation naturally flows from the known properties of fluids, and that Rumford's obſervations do not lead us to new inductions

Additional remark.

It muſt be remarked that by ſeparating the effect which took place at the firſt, when a conſiderable difference in the temperature could occaſion a quick communication, he only obſerved that which was produced when there were but very ſlight differences between ſucceſſive ſtrata of the liquid and the ice itſelf: now, when there is but a ſmall difference of ſaturation, either between chemical combinations, or between the temperatures, the equilibrium is eſtabliſhed very ſlowly, and it becomes difficult to appreciate the effects.

Experiments with mercury and heated iron

The experiments which Rumford made by plunging a ſmall cylinder of iron, heated to the degree of the ebullition of water, into water and mercury ſtanding over a ſmall piece of ice, without producing its liquefaction, only prove that when two bodies differ but little in their temperature, the equilibrium is eſtabliſhed with difficulty; for it muſt be obſerved that the iron, which had but a little ſpecific heat, and is a good conductor, muſt have loſt the greateſt part of its heat rapidly, in that part of the liquid which it paſſed gently through, and nevertheleſs have raiſed that of the liquid but little, or even that of the mercury, conſidering the maſs of it.

Other facts to ſhew that fluids are proper conductors.

But in theſe experiments of Rumford I find proofs of the property which he denies to liquids.

1ſt. In all the experiments which I have quoted, except in thoſe made with the heated cylinder of iron, the liquefaction of the ice took place in a conſiderable degree, and each part liquified ſuppoſes a quantity of heat which would have raiſed an equal weight of water from the term of congelation to 75 degrees of the centigrade thermometer.

2d. He froze the water at the ſurface of mercury cooled by a frigorific mixture; the temperature of the mercury was therefore communicated to the water, and the latter yielded its caloric to the mercury, to replace that which it loſt.

If the communication of heat was only the effect of the particles of a liquid, the mercury of a thermometer would ſcarcely change its temperature when it had arrived at the freezing point of water: in fact in ſeveral of his experiments (*Eſſay* 7,) Rumford ſuppoſes, that at this degree, the mercury no longer communicates heat: now a thermometer takes the temperature of neighbouring bodies very rapidly, and indicates it ſeveral degrees below the freezing point of water, and as far as its own congelation; then it conducts itſelf like the ſolid bodies, and its dilatations become proportionably ſmaller than the preceding.

Mercury conducts better than water, but being denſer, has leſs locomotion, ergo, &c.

Rumford has proved that the conducting power of mercury is to that of water as 1000 to 313.

This effect of the mercury, which takes the temperature of the ſyſtem in which it is placed more rapidly than the water, although it has a much greater ſpecific gravity, and is much leſs dilatable by the ſame degrees of heat, and conſequently the heat will cauſe much leſs locomotion in its particles than in thoſe of water; this effect I ſay, proves that the changes of temperature do not depend on the immediate communication and the changes of ſpecific gravity which produces the approximation of the particles of unequal temperatures, but alſo on the better or worſe conducting property of each ſubſtance.

Rumford neglects the radiant heat.

3d. Rumford paid no attention to the radiant caloric, nor did he make any allowance for it; nevertheleſs the communication of heat eſtabliſhed by its means between ſolid bodies and liquids, through the gaſes, cannot be doubted, and it may be remarked that when he brought a heated bullet near to ice and tallow, a communication of heat took place which melted the ſurface of both, without it being poſſible to attribute this communication to a circulation ſuch as he thinks is neceſſary.

Experiments of other philoſophers.

The ingenious experiments of Rumford have employed the talents of ſeveral philoſophers, who have already proved that the principles to which they led were not conformable to the true reſults of obſervation.

Nicholſon found heat to paſs downwards through a fluid.

Nicholſon, in conjunction with Pictet, made ſome experiments by which he proved, that, on heating a liquid at the ſurface, by the ſuperpoſition of a body, the heat penetrated, and raiſed a thermometer placed at the bottom of the liquid: to avoid communication by the ſides of the veſſel, a bad conducting ſubſtance was made choice of, and he aſcertained, by means of a thermometer placed in the ſame liquid near the ſide of the veſſel, that no current was eſtabliſhed which differed in the temperature: finally, the motion of the bubbles which were diſengaged, and the other appearances of the liquid, convinced him that currents were not formed.

In theſe experiments* it was proved, that liquids were different in their conducting faculty; the penetration of the heat from the top to the bottom, was five times ſlower in oil than in mercury.

—and that oil conducted much worſe than mercury.

Rumford ſuppoſed that the ſlighteſt changes of ſpecific gravity were accompanied by a locomotion, which produced a current, and he endeavoured to render it viſible, by expoſing an alkaline liquor, in which were ſuſpended very ſmall fragments of amber, which he found had the ſame ſpecific gravity as the liquid, to a change of temperature: but Thomſon has ſhown †, that the motions obſerved in theſe moleculæ were illuſory, and that, in theſe variations of temperature, which are gradual, they appear to be owing only to the difference of ſpecific gravity which they acquire, and to the adherence of air-bubbles, ſo that ſome of theſe moleculæ move in contrary directions, and run againſt each other without following the direction of the currents, he has alſo ſhown that theſe floating corpuſcules might receive different motions while the ſtrata of the liquid maintained a perfect tranquillity: he put water, tinged blue by juice of red cabbage into a glaſs veſſel; he afterwards poured clear water on it with great precaution, by means of a tube with a capillary extremity; thus he kept the two liquids ſeparate and diſtinct; he then heated the veſſel gently at the bottom: it is manifeſt that if a current had been eſtabliſhed, it would have been marked by the coloured liquid, but the ſeparation of the two liquids was preſerved unconfuſed; moreover the corpuſcules put into the firſt liquid moved upwards and downwards, and croſſed the line of ſepa-

Thomſon found that the motions of ſolids do not prove any currents in the fluids,

—the floating ſolids roſe and fell thro' two quieſcent fluids of different colours.

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* Bibl. Brit. Tom. XVIII. or Philoſophic Journal, Quarto ſeries. V. 197.

† Nicholſon's Journal, Octavo, for Feb. 1802. See alſo a memoir by this philoſopher, in the Journal for March, 1801, containing the earlieſt experimental examination of the Count's doctrine.

ration without producing the mixture of the two fluids, ſo that their various motions were not the effect of a current which carried them with it, and nevertheleſs, the heat was communicated to all the liquid. The propagation of the heat, and the agitation of corpuſcules, which have nearly the ſame ſpecific gravity, may therefore take place, independently of the circulatory motion, which is only eſtabliſhed when there is a difference of temperature of a certain intenſity between the different ſtrata of a fluid.

Murray made experiments in a veſſel of ice, containing a fluid. The heat paſſed downwards,

Murray has oppoſed Rumford's opinion with experiments ſtill more direct, and not leſs concluſive;* he placed the bulb of a thermometer in a cylinder of ice, which he filled alternately with oil and mercury; he afterwards brought a heated body near the ſurface of the liquid; the thermometer roſe ſeveral degrees in both experiments; but the heat could not have been conveyed by the ſides of the ice whoſe ſurface would have abſorbed and liquefied; no current was eſtabliſhed, for the moleculæ of the liquid having become lighter, could not take a contrary direction, and the author avoided uſing water, which contracts on paſſing from the degree of congelation to a temperature a little more raiſed: the heat muſt therefore have been communicated to the bulb of the thermometer without the eſtabliſhment of ſuch a current as is ſuppoſed to be requiſite; and that which ſerved to dilate it was only the exceſs of what had liquefied part of the ice.

—and mercury conducted better than oil.

The obſervations of Murray prove at the ſame time that mercury is a much more effective conductor of heat than oil, for the elevation of the thermometer was manifeſted by its intermedium in a much ſhorter time, and more ice was liquefied.

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* Ann. de Chim. Floreal, An. X. or Philoſ. Journal, Octavo, I. 165. 241. The experiments of Thomſon and Murray, originally appeared in our Journal, and thoſe of Count Rumford are alſo given, for which ſee the Indexes.

(To be continued.)

On

XXI.

On the Difficulty of obtaining Alumine in a State of Purity.

By R. T.

To Mr. NICHOLSON.

SIR,

YOUR Journal being open to every diſquiſition which may contribute to the progreſs of ſcience, it may not be deemed impertinent to aſk you, or ſome of your correſpondents, to point out the beſt method of preparing pure alumine.

Alumine obtained by mixing ſaturated ſolutions of alum and alkali,

You may, probably, refer me to our modern authors on chemiſtry, but I aver that the methods therein recommended do not anſwer the purpoſe. For if a ſaturated ſolution of alum of commerce, be decompoſed by a like ſaturated ſolution of a carbonated alkali, the alumine obtained is harſh to the touch, rather ſpungy, and ſtrongly adheres to the tongue.

appears to contain acid, and is ſoluble.

This earth, although waſhed as often as you pleaſe, always reddens the blue juice of the flowers of mallow, as well as of other delicate vegetable blues. It may be wholly diſſolved in about 100 parts of boiling water: and the ſolution becomes very turbid by muriate of barytes.

Dilute ſolutions afford alumine of another appearance,

If, on the other hand, a dilute ſolution of alum be decompoſed by another of alkali, a quite different product will be obtained. The alumine produced, on being deſiccated is not porous, but ſplits into pieces like ſtarch; and has, before it is nearly dry, a certain degree of tranſparency; it breaks with a ſmooth and nearly conchoidal fracture; it does not adhere to the tongue like the former, and has no earthy appearance.

likewiſe acid.

This, like the former, cannot be freed from the adhering acid. It alſo changes fine vegetable blues to red, although ever ſo much waſhed. What is the reaſon of this? Is there no method of forcing this earth from the adhering acid? or is it perhaps a characteriſtic of the earth itſelf, to redden vegetable blues. If you will pleaſe to anſwer this queſtion, or point out a better method for procuring this earth pure, you will much oblige,

Camden-Town, SIR, Your conſtant reader,
May 28, 1804. R. T.

REPLY.

THIS letter having arrived ſo late in the month, I can only for the preſent offer it to my other correſpondents.

SCIENTIFIC NEWS.

New Earth.

New earth.

A NEW earth has been diſcovered by Profeſſor Klaproth of Berlin, in an ore which was hitherto ſuppoſed to contain tungſten, to which he has given the name of *ochroit* earth, the mineral which contains it he has called ochroït, (ochroïtes.) This earth ſeems to form the connecting link between the earths and the metallic oxides. Like yttria, it produces a reddiſh coloured ſalt with ſulphuric acid; and is precipitable by all the pruſſiates; but it is diſtinguiſhed from yttria by not forming ſweet ſalts, and by not being (at leaſt much leſs) ſoluble in carbonate of ammonia, and by acquiring when ignited, a light brown colour. This earth farther differs from yttria, by not being ſoluble either by borax nor by phoſphates, with which yttria fuſes into a colourleſs pellucid bead. Its other characteriſtics, and method of obtaining will be given in our next Journal.

*Suberic Acid from Paper**.

BRUGNATELLI has obſerved, that when nitric acid is made to act upon paper, a large quantity of ſuberic acid, mixt with oxalic acid, is obtained. This proves that Fourcroy was right, in placing cork among the immediate principles of vegetables.

Eaſy and expeditious Method of preparing Copal Varniſh†.

DEMMENIE, an ingenious glaſs blower, has noticed that the ſolution of copal may eaſily be effected, by expoſing it to the vapours of alcohol or oil of turpentine. For that purpoſe an alembic may be filled ¼ with either of theſe fluids, and ſome pieces of copal ſuffered to be ſuſpended by threads in it, over the ſurface of the fluid. After having made the alcohol or oil of turpentine to boil, the copal becomes liquefied, drops into the fluid and becomes diſſolved. When no farther ſolution takes place, the whole is ſuffered to cool, and the ſolution of copal is decanted from the undiſſolved part. The varniſh has no more colour than the copal itſelf.

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* Gehlen's *Journal of Chemiſtry*: Vol. I. p. 3. page 340.

† Ibidem.

Large Piece of Amber *.

A PIECE of amber weighing 13lb. 7oz. 9ſcr. and meaſuring 318¾ cubic inches, has lately been found at Schlapacken, near Gumbinnen and Inſterburg in Germany, which is the largeſt maſs of amber hitherto found. Its colour is a pale yellow, interſected with ſeveral lines. Its value is eſtimated at about 40,000 dollars. **Large ſpecimen of amber.**

Fluoric Ether †.

15oz. Previouſly ignited and pulverized fluate of lime, were introduced into a retort containing 10oz. of highly rectified alcohol, and an equal quantity of ſulphuric acid of 1,860 ſpec. grav. and the mixture diſtilled to dryneſs. During the diſtillation, a large quantity of gas was evolved, which burnt with a beautiful blue flame, and diffuſed an odour reſembling phoſphorated hydrogen. During the combuſtion of this gas, vapours of fluoric acid were precipitated. The product which had been obtained during the diſtillation was again diſtilled to one half, and the product which paſſed over, was poured into a vial containing water. No heat was produced, nor did the two fluids mix. It was therefore ether. But as its taſte was ſour, I added to it a ſolution of potaſh; this inſtantly ſeparated a conſiderable quantity of ſilex, and the whole became converted into a gelatinous maſs. The whole maſs, on being again diſtilled, yielded pure fluoric ether. It greatly reſembled ſulphuric ether; its ſpecific gravity was 0,720; it burnt with a blue flame; its taſte was bitter, and greatly reſembling bitter almonds.

New Method of preparing Nitric Ether. By BRUGNATELLI ‡.

Nitric ether.

INTRODUCE into a tubulated retort, one ounce of ſugar, and pour over it two ounces of highly concentrated alcohol. Adapt to the retort a capacious receiver ſurrounded with cloth dipt into water; and ſecure the junctures of the veſſels by ſurrounding them with ſlips of paper only. Having done this pour through the tubulure of the retort 3oz. of concentrated nitrous acid; a violent action takes place, the ſugar becomes diſſolved, and the alcohol converted into ether, paſſes over into the receiver; its quantity is nearly equal to the alcohol employed.

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* Ibid. † Ibid. ‡ Ibid.

Accenſion of Sulphuretted Hidrogen Gas, by the Affuſion of Nitrous Acid. By Profeſſor LICHTENBERG [*].

Aſcenſion of ſulphurated hydrogen gas.

ATTEMPTING to illuſtrate the decompoſition of ſulphuretted hydrogen gas, I filled a bottle with it, capable of holding about 18oz. of water. Having done this, I poured into it at once, ¼ of an ounce of nitrous acid; a hiſſing noiſe took place, and much red vapour was diſengaged, which in order not to moleſt my auditors, I confined in the bottle, by corking it. No ſooner had this been accompliſhed, the mixture exploded with a loud report, accompanied with a blue flame[†]. The pieces of glaſs were thrown to a conſiderable diſtance, the larger ones were covered with ſulphur.

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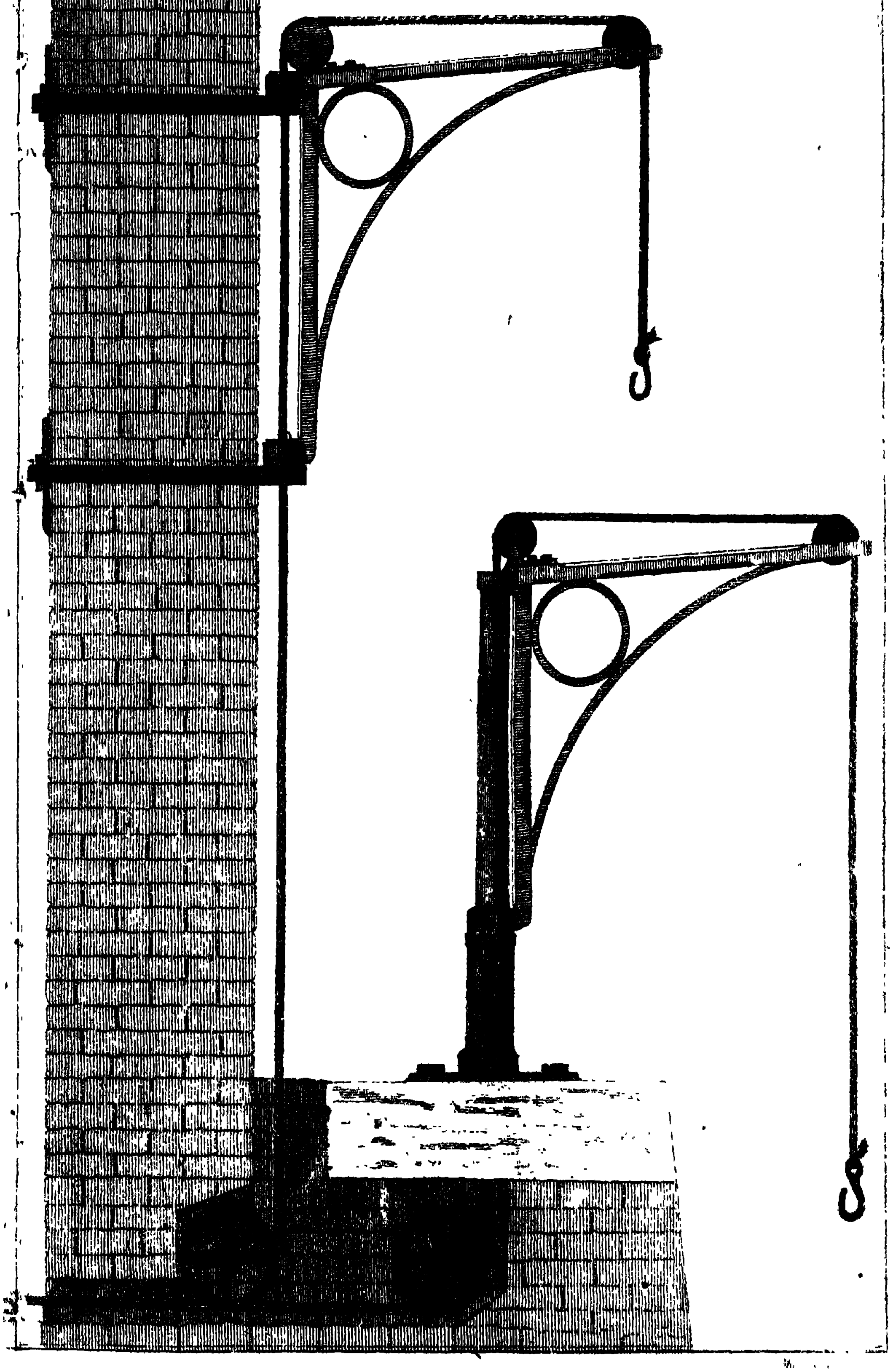
[*] Ibid.

[†] For the ſucceſs of this experiment, it ſeems to me to be neceſſary that the gas muſt be obtained by decompoſing water by means of ſulphuret of iron and an acid; for it always failed in my hands, if an earthy or alkaline ſulphuret had been made uſe of for the production of it F. A.

Mr. Bramah's new Jib.



Filtering Apparatus.

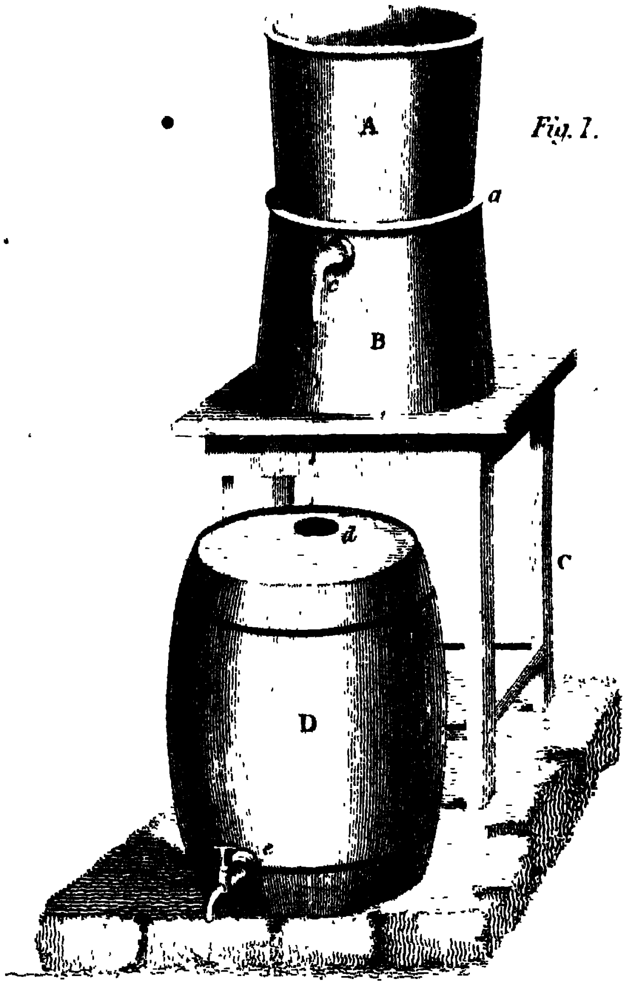
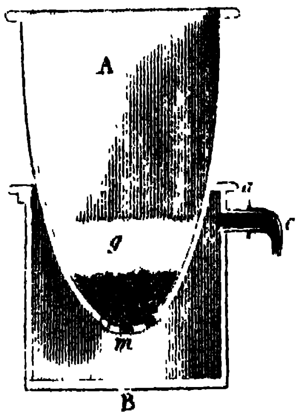


Fig. 1.

Fig. 2



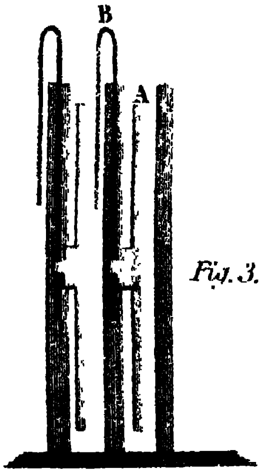


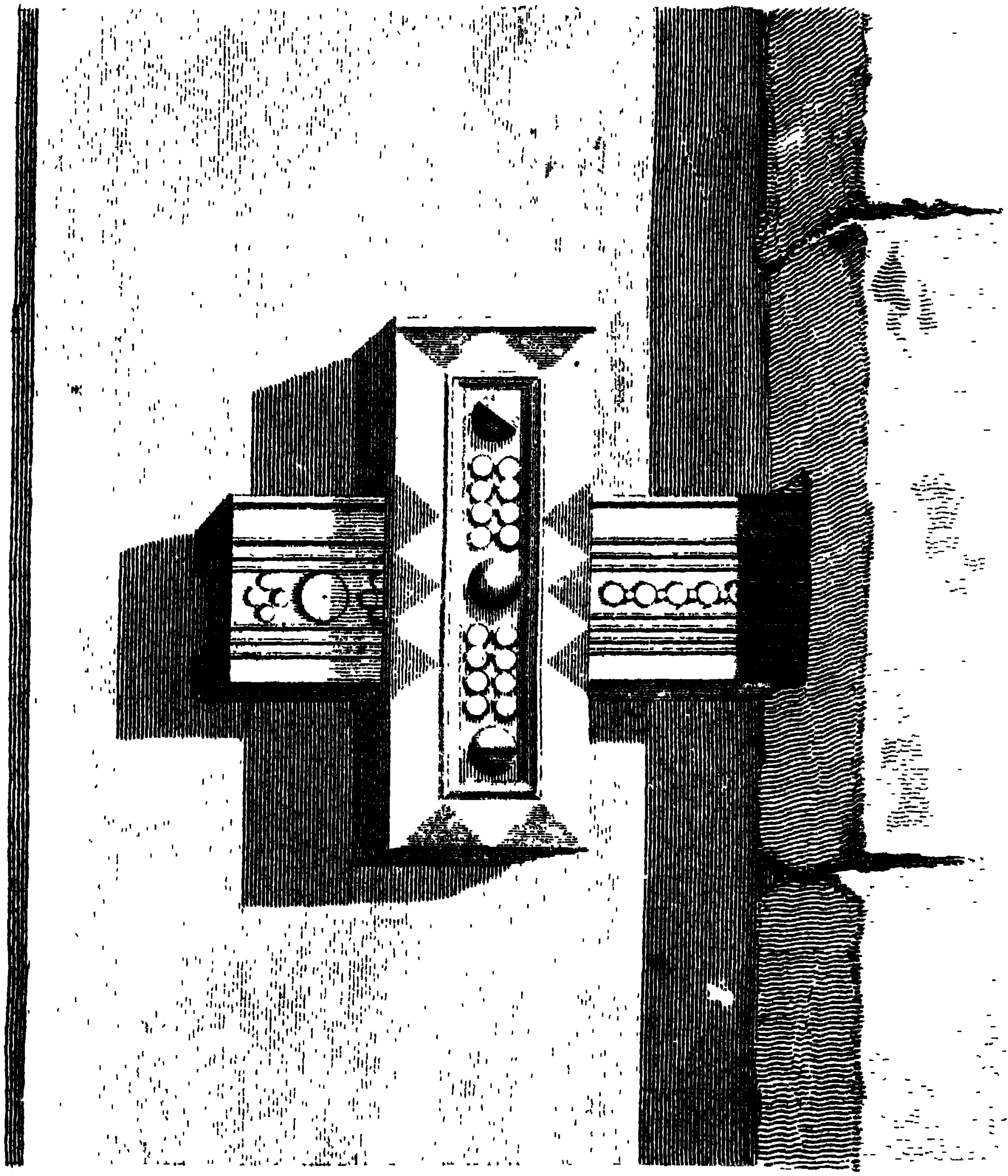
Fig. 3.

Galvanic Apparatus.

Drawn by Blunt

Engraved by Mutlow

Ancient wooden Lock,
used in Egypt, and in most parts
of the Turkish Empire.



Ancient Egyptian Lock, developed

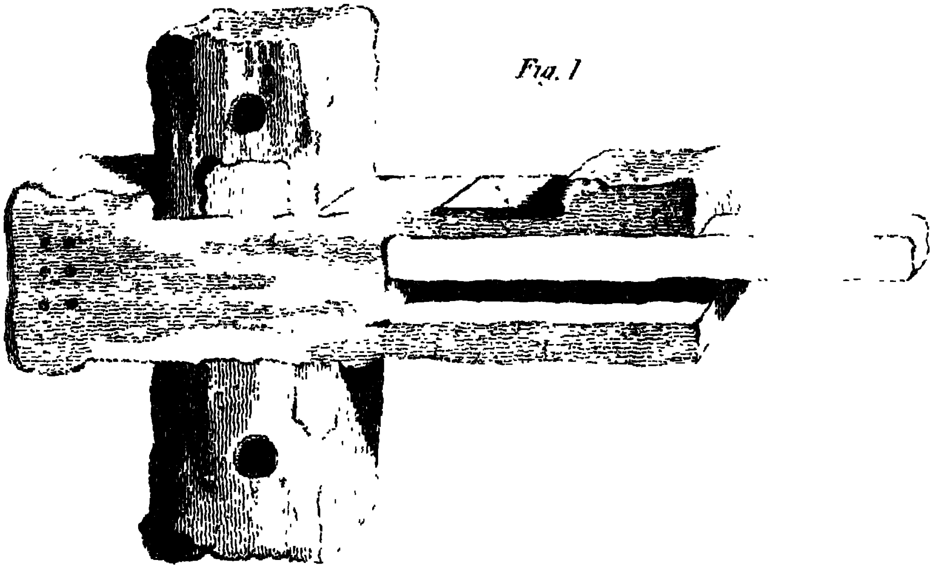


Fig. 1

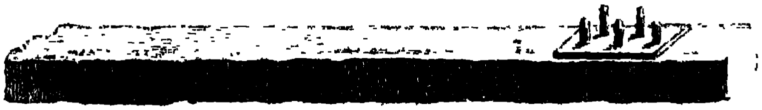


Fig. 2.

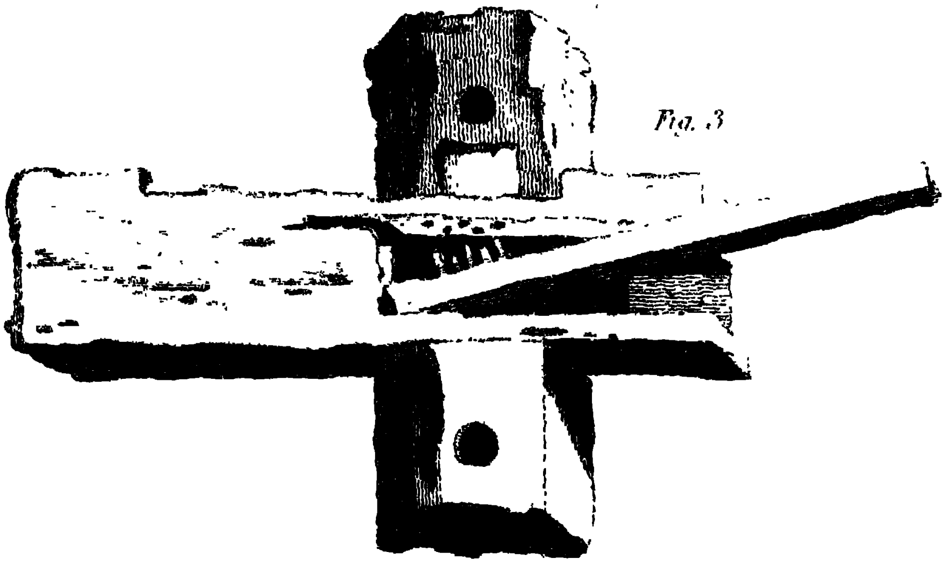


Fig. 3

A

JOURNAL

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AND

THE ARTS.

JULY, 1804.

ARTICLE I.

On the ſuppoſed Chemical Affinity of the Elements of Common Air; with Remarks on Dr. Thomſon's Obſervations of that Subject. In a Letter from Mr. J. Dalton.

To Mr. NICHOLSON.

SIR,

Difficulties reſpecting the poſition that atmoſpheric air is a chemical compound.

Dr. Thomſon's arguments in favour of that doctrine.

IN a former letter, inſerted in your Journal (new ſeries, vol. III. page 267) I endeavoured to ſhew the abſurdity of the notion of atmoſpherical air being a chemical compound of azotic and oxigenous gaſes. Beſides the difficulty, or rather impoſſibility on the one hand of conceiving how two elementary particles, conſtantly repelling each other, ſhould notwithſtanding be held together by a principle of coheſion or chemical affinity; or on the other hand, ſuppoſing the two atoms to combine, and form one centre of repulſion, how atmoſpheric air ſhould differ from nitrous gas, &c. There are a variety of facts which oppoſe the doctrine ſo forcibly that I have for ſome time wondered on what grounds thoſe who are ſtill its adherents defended it. Dr. Thomſon, in the ſecond edition of his chemiſtry, vol. III. page 316, after reviewing the opinions of different philoſophers on this head, and amongſt others my own, concludes that air is a chemical compound; he aſſigns the four following reaſons for the concluſion, which, from his extenſive ac-

acquaintance with authorities, may fairly, it is preſumed, be deemed the moſt cogent that have been offered on that ſide of the queſtion. It is the object of this communication to ſhew their inſufficiency.

1. The conſtant proportion of azot and oxigen in the atmoſphere is conſidered as an argument for their being held by affinity. So indeed it may; but it is equally in favour of my hypotheſis, and therefore nothing tending to decide the queſtion can be obtained from it. For, let part of the oxigen be abſtracted any where from the atmoſphere; then the azot may be ſuppoſed to attract the oxigen from the vicinity, and thus the equilibrium be reſtored: but it is certainly equally ſatisfactory to ſuppoſe that the oxigen in the vicinity, meeting with a leſs repulſive power from the deficient quarter, nothing prevents its diffuſion into that quarter but the azot previouſly there, which, by hypotheſis, can only *retard*, but by no means *prevent* the effect. Thus then, whether the azot *attract* the oxigen, or the oxigen *repel* itſelf, the effect is preciſely the ſame. From this fact ſimply, it is impoſſible therefore to decide the merits of either theory; but if it be found that any one gas diffuſes itſelf in any other, with nearly the ſame celerity, it will be a preſumption in favour of my hypotheſis; if otherwiſe, it may be urged that the quicker diffuſion is owing to the ſtronger affinity. I have made a great number of experiments on this head, but could not find any remarkable difference in the time and circumſtances of diffuſion of the ſame gas.

1. The conſtant proportion of oxigen and azot ſuppoſed to be regulated by affinity, or *increaſed attraction* of the abundant gas,

—but this may with more probability be aſcribed to *diminiſhed repulſion* of the deficient gas: For, &c.

2. It is ſaid the experiments of Morozzo and Humboldt ſhew that air poſſeſſes different properties from a mere mixture of its two component parts. I do not credit the experiments.—Humboldt finds a *variable* quantity of oxigen, from 25 to 30, or more per cent. in the air; whereas others who are more accurate, find but 21, or at moſt 22, and that *conſtant*. It is no wonder then, if he mix 28 oxigen and 72 azot, that the mixture diminiſhes nitrous gas more than air, and ſupports combuſtion and animal life for a longer time.

2. Humboldt and Morozzo's experiments; that a mere mixture of the gaſes differs from atm. air:—not credited.

3. Different combuſtibles are capable of abſorbing different portions of oxigen from a given quantity of air. Phoſphorus 22 per cent. Sulphur, 8, &c.—The only inferences I draw from theſe facts are, that phoſphorus will burn in oxigen of any denſity, that ſulphur will not burn in oxigen unleſs it be of $\frac{1}{7}$ of atmoſpheric denſity or more. The difference in the pheno-

3. Different combuſtibles abſorb different quantities of oxigen from common air,

mena of combuſtion in air, and in oxigen is not to be aſcribed to the combination of azot and oxigen, but to the leſs denſity of the latter, $(\frac{1}{5})$ of what a pure atmoſphere of the ſame gas would be. From an incidental but imperfect trial I made, in conjunction with Mr. Davy laſt winter, I have no doubt but iron wire would burn in common air of five times the denſity with brilliancy as in an atmoſphere of pure oxigen of common denſity. At any rate it is notorious that as the denſity of common air is increaſed, combuſtion in it becomes more vigorous. Though I have never attempted combuſtion in an atmoſphere of pure oxigen of $\frac{1}{5}$ the common denſity; I can ſcarcely doubt that the appearancees would be much the ſame as in the open air. It is probable therefore that the facts under this head, if duly inveſtigated, would turn out in favour of the hypotheſis of air being a mixture.

Aſcribed merely to the various denſities of oxigen in which they ceaſe to burn.

Propoſed experiments to decide this point.

4. " A gas no way diſtinguiſhable from common air frequently makes its appearance during the preparation of nitric acid; and Mr. Davy decompoſed nitrous oxide, by paſſing it through a red hot tube, and converted it into nitric acid and a gas, which poſſeſſed the properties of common air; now if air were a mere mixture, it is infinitely improbable that its two conſtituent parts ſhould be evolved during ſuch proceſſes exactly in the proportion that exiſts in common air."—Granted; but as the force of this argument reſts upon the *exact proportion* of oxigen and azot in the gaſes ſo evolved, that is, upon their being conſtituted always of 21 per cent. oxigen, and 79 azot, the facts ſhould be made out accordingly. Dr. Prieſtley is the only one I know of, who has particularly examined the gas produced in the preparation of nitric acid, and he found it to have *much more* oxigen than common air. Mr. Davy in his analyſis of nitrous oxide, found the gas analogous to atmoſpheric air always to contain *leſs* oxigen, though it was nearly of the atmoſpheric ſtandard.

4. Gas reſembling common air, obtained in preparing nitrous acid:

Anſwer. The preciſe reſemblance, or proportion of parts, has not been ſhewn.

The quick aſcent of hidrogen and the deſcent of carbonic acid, have been objected to my hypotheſis as facts that prove the operation of the laws of ſpecific gravities on elaſtic fluids. No doubt can exiſt that a portion of elaſtic fluid completely inſulated, as a balloon, or a bubble of carbonic acid or hidrogen, ſurrounded by a film of water, is ſubject to the laws of gravitation, and riſes or falls in elaſtic fluids on the ſame prin-

The ſtatical aſcent or deſcent of the gaſes affords no proof againſt the law of their general diffuſion.

ciple as it riſes in water; the ſame muſt be allowed when a veſſel, containing a conſiderable portion of elaſtic fluid, is ſuddenly expoſed at ſome ſurface to the atmoſphere; in this caſe, the fluids *muſt operate upon each other* for a few moments in a collected capacity, as in elaſtic bodies; becauſe the diffuſive or repulſive force by which they conſtantly tend to diſperſion, is comparatively ſlow in producing the ultimate effect, being in this reſpect exactly ſimilar to chemical affinity, the operation gradually diminiſhing as the effect draws towards a concluſion. Nothing more therefore can be inferred from the facts above-mentioned, than that gravity overpowers, and for a moment obliterates the effect of that cauſe which in other caſes ſlowly produces the diſperſion of the fluid, whether it be attraction, as commonly ſuppoſed, or repulſion, as I ſuppoſe. Chemical philoſophers have not enquired ſufficiently into the effects of expoſing gaſes in different circumſtances to the atmoſphere; all that we are uſually told is, that a jar filled with hidrogen and uncovered, loſes its gas in a few ſeconds; but if inverted, it remains nearly pure for a conſiderable time, &c. I find that a cylindric jar of 7 inches depth and 2¼ diameter, being filled with hidrogen, and inverted, loſes more than half of its gas in two minutes, and there is ſo little left as ſcarcely to explode in five minutes. If a tube, 12 inches long and ¼ inch diameter, be filled with hidrogen, and expoſed in like manner to the atmoſphere, it will loſe half its gas in five minutes, and that the ſame, whether it be held up or down or horizontal. Here we ſee effects that cannot be accounted for by gravity, that are produced in oppoſition to its agency, and where indeed it is almoſt obliterated by the action of ſome more powerful cauſe. Let the advocates for the atmoſphere being a chemical compound attend to ſuch facts as theſe, and they will ſoon find themſelves reduced to acknowledge that *all gaſes have the ſame affinity for one another*, a poſition which their doctrine ultimately tends to eſtabliſh. Indeed it is the ſame with regard to air and vapour, of water, ether, or of any other fluid; that is, all kinds of gas or mixtures of gaſes, have the ſame affinity for the ſame vapour, and even a torricellian vacuum poſſeſſes juſt the ſame affinity as any of them, judging from the quantity evaporated, and force of the vapour in a given volume. If any one doubt it, he may eaſily ſatisfy himſelf by throwing up a drop

For the ſeparate maſſes are carried by their relative gravity more ſpeedily than the diffuſion can take place.

But the diffuſion is much ſpeedier than is generally taught.

The facts, if aſcribed to affinity, would ſhew that it is the ſame between all the gaſes—and even between a gas and a vacuum.

a drop or two of ether into the vacuum of a common barometer; if the temperature be 68°, the mercury will fall 15 inches nearly; at the ſame time, if ether be admitted to a given bulk of any kind of gas, ſubject to the preſſure of the atmoſphere, the volume will be doubled, clearly ſhewing that the elaſtic vapour from the ether is the ſame in both caſes, namely, an independent fluid of 15 inches force.

I cannot diſmiſs this ſubject without obſerving, in juſtice to Dr. Thomſon, that he has entered more clearly into my views of theſe ſubjects than any other of our own country who has animadverted upon them. There are certain principles, however, which he, with moſt chemiſts of the preſent day, embraces, which are, according to my experience, decidedly erroneous. One of theſe is, *that water diſſolves air*. An excellent paper of Mr. W. Henry, on the abſorption of gaſes by water, in the Philoſ. Tranſactions for 1803, has ſhewn us ſufficiently in what light we ſhould view the ſuppoſed ſolution of air in water. Certainly air that is retained in water by mechanical force, and which always eſcapes when that force is withdrawn, cannot with any propriety be ſaid to be held by chemical affinity.

Water does not diſſolve air.

Dr. Thomſon has been miſinformed reſpecting my opinions on the expanſion of liquids. In vol. i. page 343, he gives it as my ſuggeſtion, that all liquids expand the ſame quantity from their freezing to their boiling temperatures. I never entertained ſuch an opinion; and it is certainly erroneous. My idea is, that pure and homogeneous liquids, ſuch as water and mercury, expand according to the ſquare of the temperatures from the points at which they congeal; but I have not yet found a law to regulate the *relative* expanſions of theſe and other liquids.

The author does not affirm that the expanſion of all liquids by heat is the ſame between congelation and boiling; but that any ſingle fluid expands according to the ſquare of the temperature from its freezing point.

I am your's, &c.

J. DALTON.

Mancheſter,
June 16, 1804.

II.

Eaſy Methods of completing the Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR,

Computation of ſquares:

IN the laſt number of your Journal, Mr. Councer's extenſive table of ſquares is ſtated to be deficient from No. 28261 to 29061. This deficiency may very eaſily be filled up by this rule. To the ſquare of any given root add twice that root + 1; the product will be the ſquare of the next root.

Of cubes; half the ſeries;

The table of one half the cubes in Mr. Councer's table, may eaſily be made, examined, and added to, by the following rule: Multiply the cube of any given root by 8; the product will be the cube of twice the next root.

the other half.

The other half of the cubes in the ſame table, and alſo all thoſe already found as above, may eaſily be found from any two cubes and their roots in ſucceſſion being given. Thus: From the given cube of the largeſt of the given roots, ſubtract the given cube of the next leſs given root; to the remainder add ſix times the largeſt given root, and alſo the given cube of the largeſt given root; the ſum will be the cube required; as will appear from the following example:

$$\begin{array}{rrl} & \text{Roots.} \qquad\qquad \text{Cubes.} & \\ \text{Give}\left\{\begin{array}{r} 9999 = A = \\ D = 10000 = B = \end{array}\right. & \begin{array}{r} 999700029999 \\ 1000000000000 \end{array} & \left.\right\} \begin{array}{l} \text{Required the cube} \\ \text{of the root 10001.} \end{array} \\ B - A = C = & 299970001 & \text{Remainder.} \\ 6\,D = & 60000 & \left\{ \begin{array}{l} \text{Six times the largeſt} \\ \text{given root.} \end{array}\right. \\ B + C + 6\,D = & 1000300030001 = & \left\{ \begin{array}{l} \text{Cube required of} \\ \text{root 10001.} \end{array}\right. \end{array}$$

I am, Sir,

Your moſt humble ſervant,

Eaſt Smithfield. H. G.

III.

Problems in Spheroidal Triangles. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

New problems in ſpheroidal triangles.

THE ſolution of the problem relating to the figure of the earth, in my firſt letter*, led to ſome new properties of ſpheroidal triangles, from which I ſhall endeavour, in this, to deduce a few rules, that may be applied with ſucceſs in trigonometrical ſurveys. It will be found that, though the general formulæ be complex, yet, in the caſes that occur in practice, they admit of being ſufficiently ſimplified; for not only may the terms involving the ſecond and higher powers of the compreſſion, but alſo frequently the differences between the longitudes and latitudes of the ſtations, be rejected.

Prob. I. Given two latitudes and chord of arc: Required diff. longit.

1. ‘ Having given the latitudes of two places on the ſurface of the earth, and the length of the ſtraight line or chord joining them, it is required to find their difference of longitude?’

Let λ, φ be the latitudes of the two places, D their diſtance in fathoms, a the radius of the equator in fathoms, $\frac{c}{a} = \delta$ the compreſſion at the poles, d ſuch that $\text{ſin}\ \frac{1}{2}d = \frac{D}{2a}$, and ω' the difference of longitudes of two places, whoſe latitudes are λ, φ, and diſtance d on the ſphere. Then by the formula, page 16,* the true difference of longitude ω is equal to $\omega' + Q \times \frac{c}{a} = \omega' + Q\,\delta$, if we reject the higher powers of δ, in which †

$$Q = \frac{2\,(\text{ſin}\ \lambda - \text{ſin}.\ \varphi)^2 - 2\ \text{ſin}\ \frac{1}{2}\,d^2\ (\text{ſin}\ \lambda^2 + \text{ſin}\ \varphi^2)}{\text{coſ}.\ \lambda\ \text{coſ}.\ \varphi\ \text{ſin}\ \omega'}.$$

Now becauſe in trigonometrical ſurveys, d, ω', and $(\lambda - \varphi)$ are ſmall, we may take $\frac{2\,(\text{ſin}\ \lambda - \text{ſin}\ \varphi)^2}{\text{coſ}.\ \lambda\ \text{coſ}.\ \varphi\ \text{ſin}\ \omega'} =$

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* Journal for May.

† There are ſome typographical and other errors in the paper referred to, which I have taken notice of at the end of this letter.

$$\frac{8\ \text{coſ.}\left(\frac{\lambda+\phi}{2}\right)^2\ \text{ſin}\left(\frac{\lambda-8}{2}\right)^2}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi\ \text{ſin}\ \omega'}=4\ \text{ſin}\ \frac{\lambda-\phi}{2}\times\frac{\lambda-\phi}{\omega'},\ \text{and}$$

$$\frac{2\ \text{ſin}\ \tfrac{1}{2}d^2\ (\text{ſin}\ \lambda^2+\text{ſin}\ \phi^2)}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi\ \text{ſin}\ \omega'}=4\ \text{tang.}\ \lambda\ \text{tang.}\ \phi\times\frac{\text{ſin}\ \tfrac{1}{2}d^2}{\text{ſin}\ \omega'}=$$

$2\ \text{tang.}\ \lambda\ \text{tang.}\ \phi\ \text{ſin}\ \tfrac{1}{2}d\times\frac{d}{\omega'}$; wherefore Q is equal to 4 ſin $\frac{\lambda-\phi}{2}\times\frac{\lambda-\phi}{\omega'}-2\ \text{tang.}\ \lambda\ \text{tang.}\ \phi\ \text{ſin}\ \tfrac{1}{2}d\times\frac{d}{\omega'}$, and

$\omega=\omega'-\delta.\left\{\text{tang.}\ \lambda\ \text{tang.}\ \phi\times\frac{d^2}{\omega'}-\frac{2(\lambda-\phi)^2}{\omega'}\right\}$ nearly; where the correction Q δ will be given in ſeconds of a degree, if d, ω', and $(\lambda-\phi)$, be expreſſed in ſeconds.

By ſpherics coſ. $\omega'=\frac{\text{coſ.}\ d-\text{ſin}\ \lambda\ \text{ſin}\ \phi}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi}$, whence we find, when d and ω' are ſmall, $\omega'=\sqrt{\left\{\frac{(d+\lambda-\phi)\ (d-\lambda+\phi)}{\text{coſ.}\ \lambda\ \text{coſ.}\ \phi}\right\}}$ without any ſenſible error, which value ſubſtituted in the formula $\omega=\omega'+Q\,\delta$, will give the true difference of longitude required.

When the meaſured chord is perpendicular to the meridian, ϕ is nearly equal to λ, and conſequently $\omega'=\frac{d}{\text{coſ.}\ \lambda}=\frac{D}{a\ \text{coſ.}\ \lambda}$, and $\omega=\frac{D}{a\ \text{coſ.}\ \lambda}\times(1-\delta\ \text{ſin}\ \lambda^2)$, or $\omega=\frac{D}{\text{coſ.}\ \lambda\ (a+c\ \text{ſin}\ \lambda^2)}$; but $a+c$ ſin λ^2 is equal to the radius of curvature of the perpendicular to the meridian, in the latitude $\lambda=AM$ in the figure *(Journal for May)*, which put $=R$, and there reſults $\omega=\frac{D}{R\ \text{coſ.}\ \lambda}$, which correſponds exactly with one of Legendre's theorems, *(Mem. Acad.* 1787).

Example. Let $\lambda=50^\circ\ 44'\ 23'',71$, the latitude of Beachy Head, $\phi=50^\circ\ 37'\ 7'',31$, the latitude of Dunnoſe, $D=56566,57$ fathoms, and $d=3496740$. Then becauſe ſin $\tfrac{1}{2}d=\frac{D}{2a}$, d is $=\frac{D}{a}$ nearly $=3336'',73$, $\omega'=1^\circ\ 27'\ 0'',65$, and $\omega=\omega'-3105'',74\ \delta$; in which, if we ſuppoſe $\delta=\frac{1}{252}$, we ſhall have $\omega=1^\circ\ 26'\ 47'',93$.

2. 'Having

Prob. II. Given one lat. and long. and diſtance from another place. Req. lat. of this laſt.

2. 'Having given the latitude of a place, and its longitude and diſtance from another place, it is required to find the other latitude?'

Let λ be the given and ϕ the required latitude; D the meaſured diſtance, $d=\frac{D}{a}$, ω the difference of longitude, and ϕ' the latitude on the ſphere found from λ, ω, d. Then by ſpherics $\text{coſ.}\,\lambda\,\text{coſ.}\,\phi'\,\text{coſ.}\,\omega+\text{ſin}\,\lambda\,\text{ſin}\,\phi'=\text{coſ.}\,d$, whence, when d, ω, and $\lambda-\phi'$ are ſmall, there reſults $\lambda-\phi'=\pm\sqrt{(d^2-\text{coſ.}\,\lambda\,\text{coſ.}\,\phi'.\,\omega^2)}=\pm\sqrt{(d^2-\omega^2\,\text{coſ}\,\lambda^2)}$ nearly. But by the theorem, page 16, if $\phi=\phi'+x$, we find $x=Q\,\delta$, in which the variable quantity Q is

$$=\frac{2\,(\text{ſin}\,\lambda-\text{ſin}\,\phi')^2-\frac{1}{2}d^2\,(\text{ſin}\,\lambda^2+\text{ſin}\,\phi'^2)}{\text{coſ.}\,\lambda\,\text{ſin}\,\phi'\,\text{coſ.}\,\omega'-\text{ſin}\,\lambda\,\text{coſ.}\,\phi'}=$$

$$\frac{\frac{1}{2}d^2\,(\text{ſin}\,\lambda^2+\text{ſin}\,\phi'^2)-2\,\text{coſ.}\left(\frac{\lambda+\phi'}{2}\right)^2.\,(\lambda-\phi')^2}{2\,\text{coſ.}\,\lambda\,\text{ſin}\,\phi'\,\text{ſin}\,\frac{1}{2}\omega'^2+\text{ſin}\,(\lambda-\phi')};\text{ or}$$

$$Q=\frac{\frac{1}{2}d^2\,(\text{ſin}\,\lambda^2+\text{ſin}\,\phi'^2)-2\,\text{coſ.}\left(\frac{\lambda+\phi}{2}\right)^2.\,(\lambda-\phi')^2}{\text{coſ.}\,\lambda\,\text{ſin}\,\phi'\,\text{ſin}\,\frac{1}{2}\omega'\times\omega'+(\lambda-\phi')}.$$

Now when λ and ϕ are nearly equal, this formula becomes,

$$Q=\frac{\text{ſin}\,\lambda^2.\,d^2-2\,\text{coſ.}\,\lambda^2\,(\lambda-\phi')^2}{\omega.\,\text{coſ.}\,\lambda\,\text{ſin}\,\phi'\,\text{ſin}\,\frac{1}{2}\omega+(\lambda-\phi')}=$$

$$\frac{\text{ſin}\,\lambda^2\,d^2-2\,\text{coſ.}\,\lambda^2\,(\lambda-\phi')^2}{\text{coſ.}\,\lambda\,\text{ſin}\,\lambda\,\text{ſin}\,\frac{1}{2}\omega.\,\omega+(\lambda-\phi')}.$$

Prob. III. Given one lat. with the horiz. angle and diſtance: To find diff. long.

3. 'Having given the latitude λ, the horizontal angle α, and the diſtance D, it is required to find the difference of longitude?'

Let ϕ be the latitude, and ω the longitude of the place required; with the latitudes λ, ϕ, and diſtance $d=\frac{D}{a}$, on the ſphere, to find the difference of longitude ω', and the horizontal angle α' at λ.

Then will $\text{ſin}\,\omega'=\frac{\text{ſin}\,d\,\text{ſin}\,\alpha'}{\text{coſ.}\,\phi}$, or $\omega=\frac{\text{ſin}\,\alpha'.\,d}{\text{coſ.}\,\phi}$; but by firſt queſtion $\omega=\omega'-\delta\left\{\text{tang.}\,\lambda\,\text{tang.}\,\phi'\times\frac{d^2}{\omega'}-\frac{2\,(\lambda-\phi)^2}{\omega'}\right\}$;

wherefore $\omega=\frac{d\,\text{ſin}\,\alpha'}{\text{coſ.}\,\phi}.\left\{1-\delta\left(\frac{\text{tang.}\,\lambda\,\text{ſin}\,\phi\,\text{coſ.}\,\phi}{\text{ſin}\,\alpha'^2}-\frac{2\,\text{coſ.}\,\phi^2\,(\lambda-\phi)^2}{d^2\,\text{ſin}\,\alpha'^2}\right)\right\}=\frac{d\,\text{ſin}\,\alpha'}{\text{coſ.}\,\phi}\left\{1-\delta\left(\frac{\text{ſin}\,\lambda^2}{\text{ſin}\,\alpha'^2}-\frac{2\,\text{coſ.}\,\lambda^2\,(\lambda-\phi)^2}{d^2\,\text{ſin}\,\alpha'^2}\right)\right\}.$

Now by ſpherics $\left(\frac{\lambda-\phi'}{\mu}\right)^2$ is $=\text{coſ.}\,\alpha'^2$ feré, and $\text{ſin}\,\alpha' = \text{ſin}\,\alpha - 2\,\text{ſin}\,\alpha'^2\,\text{coſ.}\,\alpha'\,\text{coſ.}\,\lambda.\frac{\phi-\lambda}{\omega}.\delta$, by theorem, page 17; or becauſe $\frac{\phi-\lambda}{\omega} = \text{coſ.}\,\lambda\ \text{cotang.}\,\alpha$ feré, the $\text{ſin}\,\alpha'$ is $= \text{ſin}\,\alpha - 2\,\text{ſin}\,\alpha'\,\text{coſ.}\,\alpha'^2\,\text{coſ.}\,\lambda^2\,\delta$; conſequently ω is $=$

$$\frac{d\,\text{ſin}\,\alpha}{\text{coſ.}\,\phi}\left\{1-\delta.\frac{\text{ſin}\,\lambda^2}{\text{ſin}\,\alpha^2}\right\}\times\left\{1+\frac{2\,\text{coſ.}\,\lambda^2\,\text{coſ.}\,\alpha^4}{\text{ſin}\,\alpha^2}.\delta\right\}.$$

From the theorem $\text{ſin}\,\alpha' = \text{ſin}\,\alpha - 2\,\text{ſin}\,\alpha'\,\text{coſ.}\,\alpha'^2\,\text{coſ.}\,\lambda^2.\,\delta$, it is manifeſt that α is nearly equal to $\alpha' + \text{ſin}\,2\alpha'\,\text{coſ.}\,\lambda^2.\,\delta$.

When α is nearly equal to a right angle, we have $\omega =$ $\frac{d\,\text{ſin}\,\alpha}{\text{coſ.}\,\phi}\left\{1-\delta\,\text{ſin}\,\lambda^2\right\} = \frac{D\,\text{ſin}\,\alpha}{K\,\text{coſ.}\,\phi}$, which correſponds with one of Legendre's theorems.

Prob. IV. Given two lats. and diff. long. To find horiz. angles.

4. 'Having given the latitudes of two places, and their difference of longitude, it is required to find the horizontal angles?'

Let λ, ϕ be the latitudes of two places, ω their difference of longitude, and α, β the horizontal angles at λ, ϕ reſpectively on the ſpheroid; alſo α, β the correſponding angles on the ſphere, which may be found from the data by the rules of ſpherical trigonometry. Then by the theorem, page 17, if we put,

$$M = 2\,\text{ſin}\,\alpha'^2\times\frac{\text{coſ.}\,\lambda}{\text{coſ.}\,\phi}\times\frac{\text{ſin}\,\phi-\text{ſin}\,\lambda}{\text{ſin}\,\omega} = 2\,\text{ſin}\,\alpha'^2\times$$

$$\frac{\text{coſ.}\,\lambda\,\text{coſ.}\frac{\lambda+\phi}{2}}{\text{coſ.}\,\phi}\times\frac{\phi-\lambda}{\omega}\ \text{feré},$$

$$N = M.\left\{\text{ſin}\,\lambda\,(\text{ſin}\,\lambda+\text{ſin}\,\phi)-\frac{1}{2}\right\}+\text{cotang.}\,\alpha'\,M^2,\ \text{and}$$

$$N' = M.\left\{\frac{1}{2}-\text{ſin}\,\phi\,(\text{ſin}\,\lambda+\text{ſin}\,\phi)\right\}+\text{cotang.}\,\beta'\,M^2,$$

we ſhall have $\alpha = \alpha' + M\,\delta + N\,\delta^2$, and $\beta = \beta' - M\,\delta + N'\,\delta^2$.

Example. Let $a = 3496740$, $b = 3477210$, $\lambda = 49^\circ\,40'$, $\phi = 50^\circ\,0'$, and $\omega = 30'$. *(See the Account of the Trigonometrical Survey, &c. Vol. 1, Page 158).* Then the two colatitudes, and the included angle 30′, will give the ſpherical angles α', β', 43° 51′ 48″,3, and 135° 45′ 16″,2 reſpectively. The remaining part of the calculation is as follows:

$2\,\text{ſin}\,\alpha'^2$ - Log. 9.98242 - $\text{Sin}\,\lambda\,(\text{ſin}\,\lambda + \text{ſin}\,\phi) - \frac{1}{2}$ 0.665036
Coſ. λ - - Log. 9.81106 - - Cotang. α'. M - - 0.432644
Coſ. $\frac{\lambda+\phi}{2}$ Log. 9.80957 - - Cotang. β'. M - - - 0.426906
Sec. ϕ - - Log. 0.19193
$\phi - \lambda$ - - Log. 1.30103 N - Log. 9.65937 — N′ Log. 9.65937
ω cour. - Log. 8.52288 206264″,8 - Log. 5.31443
δ^2 - Log. 5.49408
M - - - - Log. 9.61889
206264″,8 Log. 5.31443 $M''\delta^2$ - Log. 0.46783 — 2″,937
δ - - - - - Log. 7.74704
$M''\delta$ - - - Log. 2.68036 - 479″,025
+2 ,937
481″,962 = 8′ 1″,962 = $\alpha - \alpha' = \beta' - \beta$.

Therefore $\alpha = 43^\circ\ 59'\ 50'',262$, and $\beta = 135^\circ\ 37'\ 14'',238$.
Mr. Dalby makes $\alpha = 43\ \ 59\ \ 51\ ,\ 55$, and $\beta = 135\ \ 37\ \ 12\ ,\ 95$.

I have already remarked, that the ſum of the horizontal angles on the ſphere and ſpheroid are very nearly equal, and that they would be perfectly ſo, if we were permitted to reject the terms of the formulæ involving the powers of δ higher than the firſt. We ſhall now, by retaining the ſquare of δ, aſcertain the probable error of this theorem.

To aſcertain the probable error of the theorem; that the ſum of the horiz. angles on the ſphere and ſpheroid are nearly equal.

We have then by the formulæ, $\alpha + \beta = \alpha' + \beta' + (N + N')\delta^2$.

Now N+N′ is equal to $M \left\{ \text{ſin}\,\lambda^2 - \text{ſin}\,\phi^2 + \frac{\text{ſin}\,(\alpha'+\beta')\,M}{\text{ſin}\,\alpha'\,\text{ſin}\,\beta'} \right\}$ $= M\,(\text{ſin}\,\lambda - \text{ſin}\,\phi) . \left\{ \text{ſin}\,\lambda - \text{ſin}\,\phi - \frac{2\,\text{ſin}\,(\alpha'+\beta')}{\text{ſin}\,\omega} \right\}$; but in all the caſes that occur in practice, λ and ϕ are nearly equal, and the ſum of α', β' differs little from two right angles, wherefore the $\text{ſin}\,\lambda^2 - \text{ſin}\,\phi^2$, and $\frac{\text{ſin}\,(\alpha'+\beta')\,M}{\text{ſin}\,\alpha'\,\text{ſin}\,\beta'}$ muſt be ſmall, and the ſum of theſe is not only to be multiplied by M, which is alſo ſmall, but by δ^2, which is about $\frac{1}{90000}$. So that $(N + N')\delta^2$ is inſenſible, and therefore $\alpha + \beta = \alpha' + \beta'$.

When ϕ is nearly equal to λ, the formula may be conſiderably ſimplified. For then $M = 2\ \text{ſin}\ \alpha'^2\ \text{coſ.}\ \lambda \times \frac{\phi - \lambda}{\omega}$, and $N = M\ (2\ \text{ſin}\ \lambda^2 - \frac{1}{2}) + \text{cotang.}\ \alpha'\ M^2$; and if $\alpha = 90°$ ferè, M is $= 2\ \text{coſ.}\ \lambda + \frac{\phi - \lambda}{\omega}$, and $N = M\ (2\ \text{ſin}\ \lambda^2 - \frac{1}{2})$.

The particular ſtates of the data, when the term involving the ſecond power of δ is rigorouſly equal to nothing, may be thus determined:

In the firſt place, if $\delta = o$, or the eccentricity of the ſpheroid be infinitely ſmall, α, α', and β, β', are exactly equal to each other.

Secondly, when $M = o$, N is $= o$, but M is $= o$ when $\phi = \lambda$, or the ſine $\alpha' = o$, that is when the triangle is iſoſceles, or the directions of the places due north or ſouth of each other.

It is almoſt rigorouſly exact.

It appears then that this property of ſpheroidal triangles, firſt advanced by Mr. Dalby, and objected to by Mr. Playfair, is almoſt rigorouſly exact; and it might eaſily be ſhewn, that its application will never occaſion any material error, even in the moſt unfavourable caſe that can be propoſed. And it is not merely an elegant and curious theorem, but is highly valuable, as affording a method of determining the longitudes of places from terreſtrial meaſurements, almoſt independent of all hypotheſis. For whether the earth be an exact ellipſoid or not, any ſmall portion of its ſurface may certainly, without error, be conſidered as pertaining to one of ſmall eccentricity, which ſuppoſition is all that is neceſſary for demonſtrating the theorem.

Determination of the eccentricity at place of obſ.

Our ſolution alſo affords an eaſy method of determining the eccentricity at the place of obſervation. For if we have the latitudes and difference of longitude given, we ſhall alſo have the horizontal angles on the ſphere. But from obſervations of the pole ſtar, we may find the horizontal angles on the ſpheroid, and conſequently the difference between them; but this difference is equal to a certain function of δ in our ſolution, whence we ſhall have an equation, from which δ may be determined. Thus if α be the obſerved horizontal angle on the ſpheroid, and α' the computed one on the ſphere, we have $M\delta + N\delta^2 = \alpha - \alpha'$. Now if we reject the term $N\delta^2$ as inſenſible, we obtain a near value of $\delta = \frac{\alpha - \alpha'}{M}$, which ſubſtituted for δ in

$M+\delta$, gives $\delta = \dfrac{\alpha-\alpha'}{M+\dfrac{\alpha-\alpha'}{M}}$, very nearly. Here a queſtion naturally ariſes; *viz.* To determine the value of α' ſuch that δ may be obtained in this manner with the higheſt degree of accuracy, of which the method is ſuſceptible.

This will evidently be the caſe when M is about its maximum value. So that if we put the differential of $M = o$, and ſubſtitute the value of the differential of α' reſulting from the properties of the ſpherical triangle in this equation, we ſhall have the required value of the horizontal angle. Let us ſuppoſe λ and ω given, and there will reſult $\mathrm{ſin}\ \alpha'^2 \times \dfrac{\mathrm{ſin}\ \varphi - \mathrm{ſin}\ \lambda}{\mathrm{coſ.}\ \varphi}$ a maximum; whence $2\ \mathrm{cotang.}\ \alpha'\ \mathrm{coſ.}\ \varphi\ (\mathrm{ſin}\ \varphi - \mathrm{ſin}\ \lambda)\ d\alpha' + (1 - \mathrm{ſin}\ \lambda\ \mathrm{ſin}\ \varphi) \times d\varphi = o$. But by ſpherics,

$$\mathrm{Cotang.}\ \alpha' = \frac{\mathrm{coſ.}\ \lambda\ \mathrm{tang.}\ \varphi - \mathrm{ſin}\ \lambda\ \mathrm{coſ.}\ \omega}{\mathrm{ſin}\ \omega},$$

and $d\alpha' = -\dfrac{\mathrm{ſin}\ \alpha'^2\ \mathrm{coſ.}\ \lambda}{\mathrm{coſ.}\ \varphi^2\ \mathrm{ſin}\ \omega} \times d\varphi$; wherefore by ſubſtitution,

$$2\ \mathrm{ſin}\ \alpha'\ \mathrm{coſ.}\ \alpha' = \frac{\mathrm{coſ.}\ \varphi\ (1 - \mathrm{ſin}\ \lambda\ \mathrm{ſin}\ \varphi)}{\mathrm{coſ.}\ \lambda} \times \frac{\mathrm{ſin}\ \omega}{\mathrm{ſin}\ \varphi - \mathrm{ſin}\ \lambda}.$$

Alſo when ω and $\varphi - \lambda$ are ſmall, the $\mathrm{ſin}\ \omega = \dfrac{\mathrm{tang.}\ \alpha'}{\mathrm{cot.}\ \varphi}$. $\left\{\mathrm{ſin}\ (\varphi - \lambda) + 2\ \mathrm{ſin}\ \lambda \times \mathrm{coſ.}\ \varphi\ \mathrm{ſin}\ \dfrac{1}{2}\ \omega^2\right\}$, and $\mathrm{ſin}\ \varphi - \mathrm{ſin}\ \lambda = 2\ \mathrm{coſ.}\ \dfrac{\varphi+\lambda}{2} \times \mathrm{ſin}\ \dfrac{\varphi-\lambda}{2}$; conſequently $2\ \mathrm{coſ.}\ \alpha'^2 =$

$$\frac{(1 - \mathrm{ſin}\ \varphi\ \mathrm{ſin}\ \lambda)}{2\ \mathrm{coſ.}\ \lambda\ \mathrm{coſ.}\left(\dfrac{\lambda+\varphi}{2}\right)} \times \frac{\mathrm{ſin}\ (\varphi - \lambda) + 2\ \mathrm{ſin}\ \lambda\ \mathrm{coſ.}\ \varphi\ \mathrm{ſin}\ \frac{1}{2}\ \omega^2}{2\ \mathrm{ſin}\left(\dfrac{\varphi-\lambda}{2}\right)},$$

and $2\ \mathrm{coſ.}\ \alpha'^2 = 1$ if we ſuppoſe $\varphi = \lambda$ nearly, and $\mathrm{ſin}\ \frac{1}{2}\ \omega^2$ very ſmall.

When the horizontal angle α', therefore, is equal to 45° or 135°, the obſervations will be nearly in their moſt favourable ſtate for determining the compreſſion by means of our theorem. We might illuſtrate this method by examples taken from the trigonometrical ſurvey of Great Britain, but on reference to it I have found fewer complete ſets of obſervations than might be expected, and ſuch as are complete in every reſpect, are not well calculated for this purpoſe, the horizontal angles being nearly right ones. The obſervations at Beachy Head and Dunnoſe give $\frac{1}{148,1}$ for the compreſſion; but it muſt be remarked, that the ſtate of the data is very unfavourable in this example.

The rules which our ſolution gives for computing the horizontal angles from the latitudes and difference of longitude, will be found, I apprehend, much ſhorter than Mr. Dalby's, beſides the advantage they poſſeſs of affording us the means of aſcertaining the figure of the earth by a very ſimple proceſs, from obſervations made with the ſame inſtruments and by the ſame obſervers.

The preceding rules applicable to an ellipſoid; but the managers of the trigonom. ſurvey avoid this aſſumption.

The theorems we have been detailing, with ſome others which may perhaps form the ſubject of another letter, would give us the relative poſition of one place to another on the ſurface of the earth, were its figure an ellipſoid of known dimenſions; but as this is ſtill conſidered as problematical, the method adopted by the gentlemen who have ſo ably conducted the ſurvey of our iſland, is certainly preferable. They firſt obtain the length of a degree upon the meridian, and its perpendicular in a given latitude, and employ theſe data for computing the geographical ſituations of all the places near that parallel, and not far diſtant from a known meridian. In the ſmaller triangles the truth may be thus obtained to the fraction of a ſecond, and in the larger ones they have very ſucceſsfully employed the beautiful property of ſpheroidal triangles, which we have ſo often mentioned.

How to apply the rules.

But though we give the preference to their method of computation, I conceive the preceding rules will be found equally accurate, if we make uſe of the values of c, a, and δ deduced from their obſervations; or if we aſſume near values of them, and note the agreement or diſagreement of the computations with obſervations made at a place conſiderably diſtant from the firſt ſtation. We may thus aſcertain nearly the error of our ſuppoſitions, and then correct the intermediate ſtations. This cautious method of proceeding is rendered neceſſary by the anomalies which have been diſcovered in the meaſures of degrees in different latitudes, as well as by the general rule, which ought to be our guide in all philoſophical inquiries, to frame as few hypotheſes as poſſible, but to make accurate experiments, and infer the truth from them by fair and genuine induction.

I mean not, however, to ſupport the opinion, that the earth is not an ellipſoid; but, on the contrary, ſhould be very loth to be obliged to give up an hypotheſis, which is ſo beautiful in theory, and has ſtood its ground ſo long. Many of our objections to it may very probably ariſe from errors in obſervations, or from other cauſes which have not yet been fully examined. The remarks of Mr. Playfair on this ſubject are very ingenious, and I hope will be confirmed by the phenomena: but if not, I am convinced we have not the plea of inaccuracy to ſet up in this inſtance. One of the ſtrongeſt objections, however, has been lately done away. The degree of the earth meaſured in Lapland in the year 1736, has been found, by ſome Swediſh gentlemen ſent there for that purpoſe, to err in exceſs by no leſs than 208 toiſes. Now if we advert to the number and character of the aſtronomers who originally meaſured this degree, it will be difficult for us to ſet limits to the errors of other obſervers. Perhaps the anomaly in the degree at the Cape of Good Hope ariſes from the ſame cauſe.

The earth probably an ellipſoid;

particularly as the error of the Lapland degree removes much of the doubts entertained on this ſubject.

Fortunately, however, the great improvements, which have ſince been made in the inſtruments for aſtronomical obſervations and geodætical menſuration, afford us the means of bringing the probable errors of obſervation within very narrow limits. We may thus obtain a number of meaſures in different latitudes of equal accuracy, and by comparing them together, the queſtion about the earth's figure may be put beyond a doubt. If this compariſon ſhall be found to give different ellipſes, we ſhall then be fully warranted in rejecting the hypotheſis entirely and for ever. But till this is done, we may be allowed to adopt an hypotheſis, which is ſo ſimple, ſo good in theory, and ſupported by ſo many ſtrong arguments and accurate obſervations.

Late improvements will clear up this matter.

We have already remarked, that the degree lately meaſured in the Myſore, compared with that in France and England, gives $\frac{1}{300}$ for the compreſſion at the poles: the corrected degree in Lapland gives $\frac{1}{313}$, and that meaſured in Peru, $\frac{1}{314}$. There is a conſiderable difference between the compreſſions deduced from other meaſures, but the mean falls between theſe limits. From the beſt obſervations of the length of the pendulum that ſwings ſeconds in different latitudes, the ſame concluſion is alſo drawn; the ſecond pendulum near the pole compared

Mean compreſſion from meaſurement;

and from the pendulum;

compared with that at the equator, gives $\frac{1}{304}$ for the compreſſion. We may therefore aſſume $\frac{1}{305}$ as being very nearly its true value.

from celeſtial irregularities;

It is a curious circumſtance to find the figure of the earth, deduced from the meaſurement of lines and angles on its ſurface, confirmed (perhaps corrected), by obſervations of the ſtars and planetary bodies in the heavens, combined with the theory of univerſal gravitation. But ſuch is certainly the caſe. Among others may be mentioned two ſmall inequalities in the moon's motion, which the induſtry of modern mathematicians have unfolded. One of them was firſt taken notice of by Mayer, and fixed by Maſon at 7″,7, but was neglected by aſtronomers, as it did not ſufficiently appear that ſuch an equation ſhould ariſe from the theory, till Laplace traced it to the oblateneſs of the earth's figure. Its argument is the longitude of the moon's node, and its value has been found by Burg, from the obſervations of Dr. Maſkelyne, to be equal to 6″,8, which anſwers to a compreſſion of $\frac{1}{305,05}$. There is alſo another inequality of the moon's motion in latitude, which depends on the ſine of the true longitude, and reſults from a nutation in the lunar orbit, produced by the action of the terreſtrial ſpheroid. Burg has alſo determined the coefficient of this inequality, from a great number of obſervations, to be equal to 8″,0, which reſults from a compreſſion of $\frac{1}{304,6}$.

e. g. of the moon.

Remarks upon Newton's diſcovery and inveſtigation of this ſubject.

The preceſſion of the equinoxes, and the nutation of the earth's axis, were diſcovered by Newton to ariſe from the oblateneſs of the earth's figure. This famous problem is acknowledged to be one of the moſt abſtruſe in phyſical aſtronomy, and its complete ſolution requires the utmoſt reſources of the modern analyſis. The compreſſion thence ariſing is equal to $\frac{1}{304}$, agreeing exactly with the reſults from the two lunar inequalities, the lengths of the ſecond pendulum, and the beſt meaſurements on the earth's ſurface. It is well known that Newton failed in attempting to ſolve this problem, and ſome French mathematicians have been diſpoſed to pride themſelves on being the firſt to detect it. It ought however to be remembered, for the honour of that great man, that his miſtake did not ariſe from any error in principle, but from his taking for granted, without demonſtration, a circumſtance which appears highly probable, but is really erroneous. He ſeems to have firſt made this aſſumption in order to ſhorten his ſolution, and on finding the calculations to agree with obſervation, to have never after returned to the ſubject. The mathematicians of his time were unwilling or unable to follow him, and the queſtion remained as it came from his hands, till the middle of the laſt century. Had doubts ariſen and objections been ſtarted, the genius of Newton might have been once more rouſed to action, and continued to enlighten the ſciences to the laſt. But unfortunately no ſuch incentive was given, and Newton ſtopped ſhort in the career of his glory. The evening of his life was devoted to other ſtudies; but however uſefully he may have been employed, there are few who will not be inclined to lament that he ever laboured in the Mint or the Revelations.

Thus much I have thought it not entirely foreign to obſerve on one of the moſt remarkable effects of the oblateneſs of the earth's figure, and in juſtice to its immortal diſcoverer, the inventor of the modern analyſis, the father of phyſical aſtronomy, and the preceptor of Europe.

I am, Sir,

Your moſt obedient ſervant,

PEREGRINUS PROTEUS.

Portſmouth,
May 6, 1804.

Corrections to be made in the firſt Letter.

In page 12, line 20, after *paper* inſert *but*. In page 15, line 23, &c. divide the coefficient of the term $\frac{c}{a}$ by coſ. λ coſ. φ; alſo in page 16, lines 3 and 12. In page 16, line 9, *dele* and coſ. λ coſ. φ′; and alſo in line 14. In page 17, line 6, &c. for coſ. ω read ſin ω. In page 18, line 25, for Pc, Qc, Rc, read $P.\frac{c}{a}$, $Q\frac{c}{a}$, $R\frac{c}{a}$.

IV.

Deſcription of a ſtriking Part for a Clock, in which the Intervals between Stroke and Stroke are not regulated by a Fly, but by a Pendulum. By Mr. EDWARD MASSEY.*

SIR,

New ſtriking part for a clock.

HAVING for a number of years conſidered that a method of ſtriking a clock, at certain regular intervals, might be of great ſervice in making obſervations on the heavens, and aſcertaining the velocity of ſound, *&c.* I beg leave to lay before the Society for the Encouragement of Arts, *&c.* a ſtriking part of an eight-day clock, which I have no doubt will anſwer the purpoſe intended; and if, upon examination, the Society ſhould be of opinion that it may be uſeful, I truſt they will reward it according to its merit. They will find that the work of this model is leſs than that of the common ſtriking movements, and may be made by a common workman, with leſs expence and trouble; the weight required is alſo conſiderably leſs. The principle I act upon is the pendulum, by which I regulate the ſtroke, inſtead of the fly; the advantage of which muſt be obvious to every one. The machine conſiſts of a toothed wheel A, one pinion B, a pin wheel C, pallets D D, pendulum E, and locking detent G. The hammer-work F is as uſual, and ſtrikes on the bell at H. The weight hangs to the cord I. See *Plate* XII. *Fig.* 1 and 2, where a front and ſide view of the machinery are given, and where ſimilar letters denote the ſame parts in each view.

Deſcription of the Machinery.

I conſider it is only neceſſary for me to give the deſcription of the wheels, ſo as to be a direction to a mechanic, who wiſhes to manufacture clocks on this principle. The main-wheel A, with ſeventy-eight teeth, is to act in a pinion of eight leaves, B. The pin wheel C ſhould be large, ſo that the pins on which the pallets D and the locking G act, may be flung as far from the center as poſſible, which pins may be eight or ſixteen in number. If eight, the pendulum E ſhould be about nine inches long, and it will vibrate twice betwixt each blow of the hammer; but if ſixteen pins are put in the wheel, the pendulum muſt be about three inches long, and will make four vibrations betwixt each blow. The pins for drawing the hammer muſt be eight in number, and be fixed in a circle of about half the diameter of the aforeſaid pins. The locking-plate is on the main wheel. The ſtop is againſt the pins on which the pallets act, and may be diſcharged by a flirt piece.

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* From the Tranſactions of the Society of Arts, 1803. This invention was honoured with a reward of 20 guineas.

New ſtriking part for a clock.

As I have deſcribed the model, I beg leave to point out the method of ſtriking a clock by the common pendulum, true ſeconds, without any additional pendulum or pallets for the ſtriking part.

Deſcription.

Fix a cantrite wheel with ſixty teeth on the ſame arbor with a ſwing wheel of thirty teeth. Now ſuppoſe a ſtriking part to be made in the common way of making an eight-day clock, ſo far as the pallet pinion, leaving out the warning and fly pinions. A crank piece muſt be fixed on the pallet pinion, which muſt come into contact with the cantrite wheel, which is fixed on the ſwing wheel arbor. Then ſuppoſe the clock to be ſet a going, and the rack diſcharged, the pallet pinion will make a revolution on every vibration of the pendulum, by which means a clock will ſtrike ſeconds as true as a pendulum vibrates, which I hope will be conſidered as uſeful for the purpoſes I have deſcribed. I alſo beg leave to obſerve, that a great advantage ariſes in both the above machines, from their not being liable to foul, as the ſtroke is given by the certain and regular vibration, inſtead of the uncertain motion of the fly. Its advantage likewiſe depends on the cleanneſs of the work; and church clocks will be much benefited from the decreaſe of weight.

I am, Sir,
Your moſt obedient ſervant,
EDWARD MASSEY.

Charles Taylor, Eſq.

Hanley, in Staffordſhire,
Jan. 12, 1803.

V.

Deſcription of a very ſimple Telegraph, conſiſting of the Human Figure adapted to converſe at a Diſtance by means of Signs[*].

Preliminary obſervations.

AMONG the great advantages of which thoſe who do not enjoy ſight are deprived, we may reckon telegraphy, which it would be difficult to ſupply by another ſenſe. Such an art for the uſe of the blind would doubtleſs be very imperfect. On the contrary, the advantages which it affords to thoſe who enjoy light, appear to me to be ſo important, that it ought to be rendered more general, and brought within the reach of all men. For this purpoſe I have offered ſome notions on the ſubject. I do not pretend either to be the firſt, or the only one who has entertained them: I even believe the contrary; and, in this inſtance, they will have that in common with telegraphy in general, which, though new in its execution, was not ſo in its invention[†].

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[*] From a ſmall pamphlet in French, extracted from *Mémoires ſur les Aveugles*, &c.

[†] It is not extraordinary that the ſame invention ſhould have been thought of by ſeveral perſons. Thus the notion of telegraphy may be found in the preface to one of the German works of the celebrated Chr. Louis Hoffman, a native of Rheda, and phyſician to the Elector of Mentz, which, however, does not leſſen the merit of the French inventor. A deſcription of the telegraph invented and executed by Citizen Chappe, is found in the intereſting work of Mr. Meyer, intituled, *Fragmente aus Paris, im. IV ten Jahr der Franzæſichen Republik*; *Hamburg*, 1797. The author, who was in the telegraph office at the Louvre with Cit. Chappe, affirms, that the latter had made his diſcovery before the revolution, that he communicated it to the National Aſſembly in 1792, and that the Convention, on the report of Lakanal, decreed, July 25, 1793, the eſtabliſhment of a telegraphic correſpondence, under the direction of Citizen Chappe, as telegraphic engineer. A notion may be formed of the rapidity of the telegraphic correſpondence, by the following example, of which Mr. Meyer was a witneſs. He ſays, that during his preſence in the office at the Louvre, and at the appointed hour in the evening, enquiry was made of the office at Liſle, *by a ſingle ſignal*, if any thing new had happened in the army of the north, and the anſwer, no, was received in 88 ſeconds.

Natural telegraph.

Perſuaded of the importance of communicating our ideas at diſtances too great for the voice or hearing, I have employed myſelf in enquiries for a telegraph which ſhould be at once cheap and ſufficiently perfect to be eaſily uſed. I believe I have diſcovered it; nature herſelf has given it to all the world. This telegraph is the human body, its branches are the arms, which, with each other and with the perpendicular line of the trunk, may form a great number of figures, ſufficiently diſtinct to be readily ſeen, at conſiderable diſtances, by ſimple viſion or by the aſſiſtance of a teleſcope. It would certainly be very agreeable for two friends, living oppoſite to each other in an extenſive place or on the banks of a large river, to be able to converſe together. Of what utility might it be to the inhabitants of open countries to have a method of communication which is at once ſpeedy and requires no expence!

Method of writing the ſignals.

I hope, therefore, that the generality of readers will ſee with pleaſure that a method of communication is opened to them, ſuſceptible of being varied and brought to perfection by themſelves. In the annexed *Plate* X. are found the ſigns for all the characters in the alphabet, for the figures, and for the punctuation. To ſimplify the writing of theſe ſigns, the perpendicular and immoveable line, which repreſents the trunk of the body, may be omitted, as I have done, only indicating it in ſome ſigns or characters by a point, as in the *e* and *u;* and to write them with more rapidity, they may perhaps be joined in the manner of ſhort-hand writers.

Telegraphic ſigns by the human figure.

Three different poſitions of the right arm, and as many of the other, form the ſigns for the vowels. The right arm ſtretched out, and a little raiſed, forming an angle of about 45° with the line of the trunk, gives the ſign which expreſſes *a;* the ſame arm extended and more elevated, or horizontal, forming a right angle with the trunk, gives *e;* more elevated, and forming an obtuſe angle of about 135°, it furniſhes [illegible] The left arm extended, and forming an angle of about 45° with the trunk, gives us the ſign for *o;* more elevated, or horizontal, it ſignifies *u;* ſtill more elevated, and forming an angle of about 135°, it gives *y*.

It may be obſerved, were it only to aſſiſt the memory, that *a* and *o*, whoſe ſounds have ſome reſemblance in ſeveral words, are indicated by the ſame ſign, and likewiſe *i* and *y*, which in the French language often expreſs but a ſingle ſound; with

this

Telegraphic ſigns by the human figure.

this difference, however, that the ſigns for *a* and *i* are formed by the right arm, and thoſe for *o* and *y* by the left arm. The two other vowels, *e* and *u*, are likewiſe indicated by the ſame ſign, but with the ſame difference.

To form *b*, both arms deſcribe an angle of 45°. To form *c* and *d*, the right arm is kept in the ſame poſition, and the left arm is raiſed to the height of 90° for the firſt, and of 135° for the latter.

To form the letters *f*, *g*, and *h*, the right arm is extended horizontally, and the three different angles of 45°, 90°, and 135°, are formed with the left arm.

To expreſs *j*, *k* or *q*, and *l*, the right arm is raiſed to the height of 135°, and the ſame poſitions are repeated with the left arm as were employed for the ſix preceding conſonants.

To form *m*, the upper part of the right arm is placed in a horizontal line, and the form-arm is raiſed at the ſame time, ſo as to form a right angle with this part. To deſignate *n*, the left arm is placed in the ſame poſition. To form *p*, both arms deſcribe the preceding figure at the ſame time.

To expreſs the letters *r*, *s*, *t*, the upper part of the right arm is placed in an horizontal line, and a right angle is formed with the fore-arm, while the extended left arm ſucceſſively deſcribes the three different angles of 45°, 90°, and 135°.

For *v*, *x*, and *z*, the ſame is done with the left arm as was done, for the three former letters, by the right, which then forms the three angles of 45°, 90°, and 135°.

If it is intended to ſhew that a telegraphic ſignal is terminated, the two arms are withdrawn, ſo as to form but one line with the reſt of the body.

The right arm put at reſt, ſo that the hand is ſupported by the hip, indicates the termination of a word.

The left hand placed in the ſame poſition, is the ſign of a comm[illegible] (,).

The two arms put in this poſition, ſignify a point and a comma, (;).

By putting the right arm into this poſition, and at the ſame time making the ſign of *n* with the left hand, two points are indicated, (:).

By reſting the left arm on the hip, and making the ſign of *n* with the right arm, a point is indicated, (.).

By

Telegraphic ſigns by the human figure.

By holding the right arm in ſuch a poſition that the hand ſhall be above the head, or touch it, the point of interrogation is made, (?).

The ſame motion performed with the left arm, furniſhes a ſign for the point of admiration, (!).

The three ſigns of the termination of a word, of a comma, and of the point of interrogation, each combined with the three different angles made with the other arm, form the nine figures.

By putting the right arm at reſt on the hip, and forming the three angles of 45°, 90°, and 135°, with the extended left arm, the ſigns are given for the figures 1, 2, and 3.

By reſting the left arm on the hip, and making the ſame three angles with the right arm, the figures 4, 5, and 6, are formed.

By bringing the right arm to the ſign of the point of interrogation, and repeating the three angles with the left arm, the figures 7, 8, and 9, are formed.

To indicate zero, (0), the two arms are raiſed ſo that the hands ſhall be above the head, or touch it.

To expreſs 10, the ſigns of 1 and 0 are made without making either the ſign of the termination of a word, or of a number, or that of a comma, between theſe two ſigns. To expreſs 11, the ſign of 1 is twice made, with the aſſiſtance of the ſign for the termination of a character, or of a figure. To indicate 12, the ſigns of 1 and 2 are made, and ſo on: 10, 11, and 12 may be expreſſed at pleaſure, each by a ſingle ſign, by placing the left arm in the poſition of the point of admiration, and by ſucceſſively forming with the right arm the three angles of 45°, 90°, and 135°. It would not be difficult to deviſe a great number of ſigns to expreſs 13, 14, 15, and many other numbers, each by a ſingle ſign.

Mechanical addition for great diſtances.

If it be intended to make uſe of this natural telegraph at the diſtance of one, or even of ſeveral leagues, or to do without a teleſcope at leſs conſiderable diſtances, it is only neceſſary to add to each natural arm an artificial one, that is to ſay, an oblong frame, of the breadth of a foot or thereabout, and of the length of one or more yards. This frame muſt be covered with oiled ſilk, or any other ſtuff of a dark colour, and muſt be provided with a handle to hold and direct it. Our telegraph would here no longer be called *natural*, but it is ſtill among the moſt ſimple of the artificial. I am of opinion that this kind of telegraph may be employed with very great advantage: it is ſimple and cheap; it may be uſed in all directions, and is removed with eaſe. In this latter point of view, it would doubtleſs be well calculated for the formation of moveable telegraphic lines, or what might be called a flying telegraphy, which in my opinion would be very uſeful in war, to keep up a quick and conſtant communication between the different bodies, and with the fixed and common telegraphic lines. A machine of eaſy carriage, and calculated to raiſe a man to a conſiderable height, would, no doubt, contribute to render the flying telegraphy more perfect. The telegraphs with the frames may be uſed by night as well as by day, by adding lanterns to them, as in the common telegraphs. One muſt be placed in the middle, that is to ſay, on the breaſt of the perſon who forms the telegraphic ſignals with his arms and the frames, one at the handle, and one at the extremity of each frame. To prevent the perſon from being affected by the vapours of the lantern in the middle, it ſhould be placed at a ſmall diſtance before him; and when the movements are intended to be viſible on both ſides, it would be ſtill better to place this lantern on the head, or to put one on each ſhoulder.

May be uſed by night as well as by day.

Perhaps this kind of telegraph might be advantageouſly employed to eſtabliſh immoveable telegraphic lines, in every caſe in which the telegraphs employed at preſent would be too expenſive. The ſame telegraph might frequently ſerve for ſeveral lines in different directions. By means of this telegraph a communication might be maintained, with little coſt, even in ſmall diſtricts, or in the provinces and departments, with the capital or the chief place, and the other conſiderable points. Of what importance may it be to be able to indicate inſtantaneouſly conflagrations, inundations, or other events which require ſpeedy ſuccour.

It would be well to place the telegraph, or the perſon who makes the telegraphic ſignals, on a kind of ſtool, to facilitate his movements. Placed in this manner, with his arms and the frames he could make a prodigious number of figures, ſufficiently diſtinct not to be confounded with each other.

VI.

Account of a Fact reſpecting Water heated in a Boiler of Stone; with Obſervations. By Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

DEAR SIR,

THOUGH it muſt be admitted by every one who has attended to the critique contained in your note on the experiments of Count Rumford, in your firſt volume * of the Philoſophical Journal, concerning the non-conducting power of fluids with regard to heat, that it is an erroneous concluſion, and which ſeveral experiments, particularly that of Mr. Murray, have ſince confirmed; yet I cannot be quiet without informing the gentlemen who have ſo conſtantly oppoſed the Count's doctrine, of a circumſtance which accident brought forth on a very large ſcale, about forty years ago, in the county of Cornwall. Fluids not ſtrictly non-conductors. Incident.

It had long been a deſideratum in the mining intereſt in that county, to reduce the conſumption of fuel in draining the mines by the means of ſteam engines, and every expedient that carried any tolerable face of probability was brought to a trial. Attempt to reduce the conſumption of fuel in working ſteam engines.

Among the reſt it was ſuggeſted, that as, in the ſeveral operations of ſmelting the produce of the mines, much heat muſt be carried off, from the intenſe fires of their furnaces, which might be employed to ſome purpoſes requiring but a ſubordinate temperature; a reſolution was formed by a company of gentlemen, with Mr. John Weſton at the head of it (to whom mining was an ungovernable hobby-horſe), to erect an engine on a copper mine in the pariſh of Camborne, and to put up a ſet of furnaces ſo attached to the engine that they might avail themſelves of this ſuperabundant heat to raiſe the ſteam. Trial to work by the ſmelting heat.

To effect this they had their engine-boiler made of maſonry, of what is called in Cornwall *moor-ſtone*, well wrought and put together with Aberthaw lime, which has the property of ſetting A ſtone boiler connected with water-lime, &c. heated by copper tubes.

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* I. Page 291, Quarto Series.

like the Dutch traſs *; and to convey the heat, two or three copper tubes were placed in it from end to end, and the furnaces connected to one end of the boiler, and the engine at the other; which, though it was a prodigal way of ſaving heat, yet it was competent enough to raiſe ſteam for the uſe of the engine in certain caſes.

When the water boiled, a cock near the bottom gave water, but little heated.

At the bottom of the boiler was placed a cock, as uſual, to tap it as occaſion required, either when it was neceſſary to clean it, or in caſe it ſhould be over-charged with water, which I think was the caſe which brought the following fact to light; namely, that when the fire had been lighted, and the heat had to circulate through all this maſs of ſtone-work, and the water brought to boil, and the engine had been at work for the firſt time, which is uſually attended with many unforeſeen delays and hindrances, the cock being turned, the water was not hotter than to admit the hand without any painful ſenſation of heat. Many deep and grave hypotheſes were formed on this extraordinary diſcovery, which, for the ſake of the profeſſion at this advanced ſtate of ſcience, I muſt forbear to mention. But at that period many a valuable truth had come and gone like a worthleſs mendicant, whom few regarded, and whom none would receive.

The tubes were near the ſurface, and the veſſel a bad conductor.

The moſt ſtriking circumſtances in the detail of this diſcovery are, that the temperature downward (for the tubes were near the ſurface of the water) muſt be greatly accelerated by the conducting power of the ſides of the boiler, and the agitation of the water in a ſtate of ebullition: notwithſtanding all, it muſt have occupied four or five hours to bring the lower part of the water to the temperature of 90°, or perhaps 100°; and no doubt can be entertained but that they deemed it a matter of no conſequence as to the place of the tubes, ſo long as they were covered with water; believing with every body elſe, that the whole maſs would attain the boiling point at the ſame time.

Hail the man who throws if but one grain of truth into the ſcale againſt popular prejudice or vulgar error; and notwithſtanding the Count did not obtain the extreme of preciſion in this particular inſtance, he has laid the foundation for all the ſubſequent experiments which have gone to confirm or reject

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* This lime is found on the beach on ſome part of South Wales.

the poſition he advanced. This I may be permitted to obſerve, without the imputation of detraction from thoſe whoſe labours and talents demand my reſpect.

I am, Sir,

Your very obedient ſervant,

City Road, J. C. HORNBLOWER.

June 11, 1804.

VII.

Facts and Obſervations tending to elucidate the Theory of Galvaniſm. By A CORRESPONDENT.

General effects and theory of galvaniſm.

THE pile of Volta has diſcloſed many new properties which were not known to belong to the two powers of the electric fluid, for by its operation, two ſets of chemical phenomena are produced, differing eſſentially from each other, thereby determining the real or poſitive nature of each power. It has introduced electricity as a chemical agent, and has ſhewn us that it acts as ſuch in its two-fold character very generally throughout nature. Electricity has hitherto been conſidered moſtly as an adventitious power excited upon the ſurfaces of bodies, it is now known to attach itſelf to, and to enter into combination with various ſubſtances.

If the ſpinal marrow of a frog be coated with tin foil, and the muſcles of the lower extremities be laid bare, if the frog, thus prepared be drawn through water, at the bottom of which there is a piece of ſilver, when the tin foil touches the ſilver, the limbs of the frog are thrown into ſtrong convulſions. If the tin foil be kept ſteadily at the firſt place of contact, the convulſions ſoon ceaſe; but if it be removed to the ſmalleſt diſtance from where it firſt touched the ſilver, and is again in contact with it, the convulſions are renewed with their original violence. The ſame thing happens when the ſilver is connected with the muſcles by means of a metal, though that metal touch the muſcle in a point, as when the water acts as the conducting arch.

Theſe experiments ſhew that there is a power in each aſſignable particle of the metal which excites the action of the muſcles, and that nothing but the contact of the metals ſeems neceſſary for the production of the effect.

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If the power which excites the motion of the muſcles be electricity, it is in a ſtate not to be conducted from one particle to another, for the ſame effect is renewed whenever the tin-foil comes in contact with another part of the ſilver, however contiguous it be to the one at firſt touched. We find farther that the ſuperficial particle does not take off the power of the one immediately beneath.

It is thus, I apprehend, that the renewal of ſurfaces gives a permanency to the powers of the pile, and in proportion to the quickneſs of ſucceſſion in the preſenting new ſurfaces, the power of the pile is found to increaſe.

It is needleſs to explain theſe appearances by the hypothetical principle of Mr. Lavoiſier's oxygen, ſince Mr. Volta has ſhewn that the metals themſelves are electro-motors, even without the intervention of water, or the action of acids or alkalis. Taking it for granted then that the animal muſcles are a ſpecies of electrometer, we may conclude that ſome diſturbance of electricity takes place when two metals are brought in contact, and a circuit is made. This diſturbance varies according to the ſubſtances employed. Mr. Volta has found this to be the caſe, for he has rendered this ſmall portion of electricity perceptible by accumulating it on his condenſer. The pile of Volta accumulates the power which is called forth by the contact of two metals, and enables us to apply it in ſuch accumulated ſtate in various chemical experiments.

One of the moſt ſtriking effects is the diſengagement of two elaſtic aeriform fluids from water, by introducing wires, from which are produced the two ſtates of the electric fluid. Thus it happens that the wire connected with the zinc produces, when immerſed in water, one gas, and the wire connected with the ſilver or copper plate another gas. The properties of theſe gaſes are perfectly diſtinct, and evince that the producing powers have peculiar effects, for the water is common to both. What takes place from the wires in water at the extremities of the pile is more or leſs apparent between the plates themſelves, where wetted paper is interpoſed, for there the action of the gaſes is to be ſeen in their effects on the metallic plates. The effect produced by the pile may be greatly increaſed by arranging it in the following order; ſilver, wet paper, zinc, a plate of glaſs, &c. and by placing wires on each ſide of the glaſs, and putting thoſe into water; from each

General effects and theory of galvaniſm.

pair of wires the two airs will be produced. Might not Mr. Wilkinſon's experiment of burning charcoal be in this manner greatly increaſed in ſplendour?

In the proſecution of the preſent inquiry, I ſhall eſpecially notice the two different effects produced in water by the wires of Volta's pile. It is known that theſe two wires throw out two powers ſimilar to what are called poſitive and negative electricity; and as water is common to both, I am led to conclude that the two gaſes are produced in one inſtance by the union of one of theſe powers with water, and in the other by the other power and water, and to conſider each gas as thus compounded, becauſe we have no evidence of any other power, principle or ſubſtance being acceſſory to the production of either of the gaſes. Volta's pile cauſes combuſtion in atmoſpheric air, and therefore vital air is abſorbed; it cauſes another ſpecies of combuſtion in vacuo, becauſe the two principles of fire are ſupplied by it. Volta's pile does not act in vacuo, becauſe the reſiſtance neceſſary to accumulate the powers is withdrawn in the ſame manner as the Leyden jar does not charge in vacuo.

If the gas which is produced from one of the wires communicating with the pile in water be united and inflamed with the other in a juſt proportion, the water which is common to both is re-produced, and common fire is generated in great abundance; now as we have had no evidence of the preſence of fire till this point of time, does it not appear that theſe two principles which are thrown out from the wires of the pile, are by their union transformed into ordinary fire, and does it not appear in this experiment that ordinary fire is generated by and compounded of theſe two powers?

Lavoiſier has triumphed over the friends of Phlogiſton. He overturned a ſyſtem which was built upon the aſſumption of a principle, upon a mere name of hypothetical entity, endued with attributed or imaginary properties. But in the place of the ſyſtem he deſtroyed, he has erected one of his own. Has he not built upon the ſame weak foundation with his late adverſaries? Does not his ſyſtem reſt altogether upon the aſſumed diſtinct exiſtence of things, the diſtinct exiſtence of which cannot in any way be evinced? Do not all his ſolutions of chemical phenomena depend on ſuch aſſumption? May not all theſe phenomena be explained with equal eaſe, nay more ſimply, by referring them to the action of known agents?

General effects and theory of galvaniſm.

If we examine Mr. Lavoiſier's theory of combuſtion, we ſhall find that it depends upon the aſſumption of a principle whoſe exiſtence has never been proved by direct experiment. It is the union of the baſis of oxygen gas with the combuſtible body which in every inſtance is the cauſe of the combuſtion. We have ſeen that this gas is produced by poſitive electricity and water, and we conclude that the ponderable part of this gas is water, becauſe we can perceive the agency of no other principle or ſubſtance. The ſimple condenſation of oxygen gas, according to Mr. Lavoiſier, occaſions the evolution of heat and light. But little heat is excited by the mechanical condenſation of this gas; but we find that when a chemical union takes place between a combuſtible and the gas, a great portion of heat and light is diſengaged.

The accenſion of charcoal and metallic bodies by the galvanic battery, even in vacuo, ſeems to ſhew that the electricities thus produced are the peculiar agents in the phenomena of combuſtion. They ſeem not only to be the exciters of combuſtion, but they furniſh with water thoſe gaſes by which combuſtion has been conceived to be upheld.

The theory of the Lavoiſierian ſchool, of the combuſtion of hydrogen gas, in conjunction with oxygen gas, is as follows: "When both gaſes are mixed at a lower temperature than that of ignition or red heat, the attraction of their reſpective baſes to the caloric is, in that caſe, ſtronger than their attraction to each other, and therefore they are not decompoſed. But in the heat of ignition, their baſes, namely *concrete* oxygen and hydrogen, again attract each other more ſtrongly, and hence they unite to produce water; and both the caloric and light by which they are retained in their aerial form, are again diſengaged from them, and conſtitute the fire."

Now where is the evidence of the heat and light being neceſſary to the formation of theſe gaſes, when they are produced from the wires connected with the extremities of the pile, and placed in water? Alſo during the oxidation of a metal in the moiſt way, the hydrogen diſcharged is at firſt very hot, ſo as to heat immoderately the tubes uſed in collecting it. If the condenſation of oxygen gas be the cauſe of the production of heat and light, how comes it that charcoal and nitre deflagrate together with the production of ſo much light and heat, although the compound formed, namely, carbonic acid gas, occupies ſo much more ſpace than the ſubſtances from which it is formed? Does it not ſeem an aſſumption to ſay that the two conditions of oxygen in the aeriform ſtate, and oxygen in the ſolid ſtate, as in nitre, ſhould hold the ſame quantity preciſely of heat and light? This fact diſproves the opinion that the production of heat and light is commenſurate with the condenſation that takes place in combuſtion.

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An eſtabliſhed law of chemical affinity would be reverſed, if oxygen, when in combination with another ſubſtance, were to have a greater, or even the ſame attraction for the matter of heat as it has when oxygen is ſimply combined with the matter of heat.

There are many inſtances of combuſtion, deflagration, &c. wherein the compounds formed ſeem to hold more heat in their compoſition after than before combuſtion. When gun-powder is inflamed, does not the production of ſteam ſeem to ſhew that a great quantity of heat is *generated* during the exploſion. If the matter of heat was held to the particles of the component parts of gun-power, in conſequence of their great capacity for containing it, ſhould we have any reaſon by analogy to ſuſpect that the electric ſpark would entirely reverſe the order of the exiſting attractions? Have we not evidence of the accumulation of one power of the pile in nitrous and muriatic ſalts, for in the formation of their acids we do not find combuſtion to be neceſſary, and there is no known expenditure of this their igneous principle. Theſe are the ſalts which cauſe the combuſtion of inflammable bodies, and burn them, in conſequence of their both containing igneous and inflammable principles. Thus does it appear that fire is generated during combuſtion.

From all the obſervations and experiments I have made on this ſubject, I conclude that water is ſtill to be conſidered as a ſimple ſubſtance, that its two aſſumed component parts are non-entities, that fire is generated in every ſpecies of combuſtion, whether by acids or otherwiſe, and that heat is generated in the lungs during reſpiration. I take the liberty alſo to ſuggeſt, that the admiſſion of the hypothetical ſubſtances of the Lavoiſierian ſyſtem may have retarded the progreſs of ſcience, by diverting the mind from real objects. Analogies drawn from imaginary data muſt tend to perplex and confound, and thus do names ariſe without exiſting realities. Perhaps it would be as proper to queſtion the exiſtence of azote, if oxygen and hydrogen were to fail, as it was to admit it, from the ſuppoſition of the exiſtence of thoſe two ſubſtances.

A CORRESPONDENT.

VIII.

Experiments with the Electric Pile, by Mr. Ritter, of Jena. Communicated by Mr. Oersted.*

WHILE the great inventor of the electric pile was proving the identity of galvaniſm and electricity, many of the philoſophers of Germany were buſied in the ſame purſuit. On this ſubject the celebrated Ritter undertook a very extenſive ſeries of experiments, the reſults of which are ſo remarkable as to merit attention, even after the publication of the labours of the philoſopher of Pavia.

To compare galvaniſm with electricity we muſt attend to the kind, chemical action, ſpark and ſhock.

In order to compare galvaniſm properly with electricity, four different phenomena are to be diſtinguiſhed; the kind of electricity, chemical action, ſpark, and ſhock.

1. Kind.
The pile has a poſitive and a negative pole: and the intenſities decreaſe gradually from the exremities to the centre.
But the whole pile becomes negative, when the poſitive pole is made to communicate with the earth by means of a conductor, and *vice verſa*.
Yet this change in the kind of electricity does not affect its chemical action.
A collateral fact is the increaſe of the action by adding a ſalt to the water without changing the kind of electricity.

As to the kind of electricity, it is known to every perſon, that the pile has two electrical poles, one poſitive, the other negative. On a more attentive examination we diſcover, what was not difficult to foreſee, that the reſpective intenſities are ſtrongeſt at the extremities of the pile, and regularly decreaſe from the extremity to the centre, where the intenſity is at its minimum. But it was never yet ſuſpected, that the whole pile becomes negatively electrified, when a communication is made between the poſitive pole and the earth, by means of ſome conducting ſubſtance; and on the contrary, the whole pile becomes poſitive; when the electricity is abſtracted from the negative pole. We have here a phenomenon, that ſhews the theory of electricity to be ſtill in its infancy: When the ſtate of t[illegible]le is thus changed, its chemical action is not deſtroyed, b[illegible] continues as before. This fact is perfectly conſiſtent with the augmentation of the chemical action of the pile, on the addition of ſalts, acids, or alkalis, to their wet ſtrata, in which caſe the kind of electricity remains conſtantly the ſame as when ſimple water was employed.

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* *Journal de Phyſique*, December, 1803, Vol. LVII, p. 401.

The aſſertion of Volta, ſince repeated by Van Marum and Pfaff, that the electrical pile charges a jar or a battery inſtantaneouſly, requires ſome limitation.

Volta, Van Marum, and Pfaff, ſay, that the pile changes a jar inſtantaneouſly.

With regard to common piles, the aſſertion is true: but if the pieces of paſteboard, inſtead of being thoroughly wetted, contain but very little moiſture, that of the damp air for example, the action takes place much more ſlowly. At firſt, indeed, the action is ſtill tolerably prompt, but in proportion as the paſteboards loſe their moiſture, the action becomes gradually ſlower; ſo that a pile of ſix hundred pairs of zinc and copper, uſed immediately after it was made, was ten or fifteen minutes charging a battery of thirty-ſix ſquare feet to the ſame degree, to which it would have charged it inſtantaneouſly, had it been conſtructed with pieces of paſteboard thoroughly wetted. Each of the wet ſtrata too may be compoſed of a piece of glaſs, armed on each ſide with wet paſteboard; and ſuch a pile of ſix hundred pairs would require twelve hours to charge the battery to the ſame degree, as would be done by a common pile, with a ſolution of a ſalt, in an imperceptible ſpace of time. The law of this retardation therefore is, that the action of a pile is more ſlow, in proportion as it is a worſe conductor.

This is true with the common pile; but if the paſteboards be little moiſtened, the jar will be charged more ſlowly.

Such a pile of 600 pairs was 10 or 15 minutes charging a battery of 36 feet as high as it would have done inſtantaneouſly with thoroughly wet paſteboard.

By interpoſing glaſs armed with wet paſteboard between each pair of metals, the time would be protracted to twelve hours..

The action of the pile therefore is quick in proportion to its conducting power.

Ritter has made a great number of experiments in particular, which prove, that the ſtate of the pile, *on all occaſions*, obeys the ſame laws, as that produced by the electrical machine: but we cannot here enter into particulars, without exceeding the bounds allowable to an abſtract.

Ritter has proved by many experiments that the electricity of the pile obeys the ſame laws with reſpect to kind as that produced by the machine.

It is a well known fact, that electricity produces the ſame change in water as galvaniſm. Ritter has ſhown, that poſitive electricity, like poſitive galvaniſm, diſengages from it oxigen gas; and that negative electricity, like negative galvaniſm, diſengages from it hidrogen.

2. Chemical action.

Galvaniſm and electricity produce the ſame change in water; each in the poſitive ſtate extricates from it oxigen; in the negatives, hidrogen.

Inquiries into the action of the pile on metals have taught him, that its negative pole diſpoſes them to combine with the hidrogen of water, as the poſitive pole diſpoſes them to combine with oxigen. The hidrogenation has different degrees with reſpect to the ſame metal, as well as the oxidation. Silver with a large quantity of hidrogen aſſumes the ſtate of gas: with a ſmaller quantity it remains ſolid. He alſo found, that the electricity does not produce oxigenations and hidrogenations in the humid way alone, but in the dry way alſo. The oxida-

The negative pole of the pile diſpoſes metals to tion produced by the poſitive pole is eaſily obſervable. Nothing more is neceſſary, than to arm it with a leaf of gold, and the negative pile with a bit of charcoal; when, on bringing them into contact, the gold leaf will burn with a bright flame, and the charcoal remain untouched. If the charcoal be placed in contact with the poſitive pole, and the gold leaf in contact with the negative, the charcoal will burn, and the gold melt. The hidrogenation produced by the negative pole is leſs diſtinct, ſo that it is ſeldom perceived: but facts may be adduced however, that prove its exiſtence. If a little veſſel of iron or platina, filled with mercury, be placed in contact with the negative pole, on touching the ſurface of this fluid metal with the poſitive conductor, we obtain a point or circle of a powder very different from the black oxide of mercury, which is produced when the mercury is placed in contact with the poſitive pole, and touched with the negative conductor. The oxide produced in the latter caſe arranges itſelf in the figure of little ſtars, equal to thoſe produced by poſitive electricity with the powder: and the circular figures on the mercury at the negative pole are likewiſe equal to thoſe produced by the powder electrified with the negative conductor.

In a pile the poles of which are not made to communicate by means of a conducting ſubſtance, the chemical action of the ſtrata compoſing it is very unequal. The plates of zinc are oxided leſs in proportion to their diſtance from the poſitive pole; ſo that thoſe neareſt the negative pole have frequently no traces of oxidation, and ſeem rather to have been protected from the action of the water by which they are wetted, than attacked by the action of the pile. This may be rendered ſtill more evident by placing every fifth pair in contact with an iron wire, the other end of which is plunged in water. In this experiment the oxidation of theſe wires will be in the inverſe ratio of their diſtances from the poſitive pole; in the centre the wire will not be more oxided than another ſimply plunged in water for an equal length of time, and all the wires beyond this will be ſtill leſs oxided. Hence it is evident, that another action, the reverſe of oxidation, has taken place.

Of all the effects of the pile, its action on the human body has been leaſt examined. The ſhock, or rather palpitation, that it excites, has been conſidered as too ſimple, to be ſubjected to ſtrict inquiry; and the flaſh, as well as its action on

the

combine with the hidrogen of water; the poſitive with its oxigen.

The hidrogenation of metals has different degrees, as well as their oxigenation.

Silver highly hidrogenated becomes a gas: leſs ſo, remains ſolid.

Galvaniſm hidrogenates and oxigenates metals in the dry way alſo.

If the poſitive pole of the pile be armed with gold leaf, and the negative with a bit of charcoal, on making the contact, the gold leaf will burn with a bright flame, and the charcoal remain unaltered: if the ſituation of the two be reverſed, the charcoal will burn, and the gold melt.

The hidrogenation is leſs diſtinct: but if mercury, in a veſſel of iron or platina, be placed in contact with the negative pole, and its ſurface touched by a conductor from the poſitive, a ſpot or circle of a powder different from the black oxide, which is produced under the oppoſite circumſtances, will be found on the ſurface.

the tongue, has drawn but ſlight attention. It is true theſe inquiries, like all others relating to organized beings, are very difficult, particularly when they concern an action that is frequently injurious to the living ſubject. Ritter has more than once paid for the following diſcoveries, by long and even dangerous fits of illneſs.

It is well known, that the ſkin, being a bad conductor, muſt be wetted, to make it a good one: it is likewiſe found in practice, that a ſurface of conſiderable extent muſt be wetted and armed with metal, to have all the poſſible effect of a pile. The reaſon is not difficult to diſcover, though it may lead to many important conſequences: we have only to advert to the known fact, that conductors can only convey a quantity of electricity proportionate to their ſurface; whence it follows, that, to produce the greateſt effect, a conſiderable extent of ſkin muſt be made a good conductor. If one of the ſurfaces wetted and armed with metal, which is touched by the conductors of the pile, be larger than the other, the ſenſation is leſs diſtinct than that which takes place on the ſmaller, where there is a more perceptible, and often painful ſenſation: ſo that we are maſters of the magnitude of the effect that we would produce on any part of the body, a very important circumſtance in employing galvaniſm medicinally.

The following is an application of what has juſt been ſaid. All the difference between the ſhock obtained from the pile, and that received from a jar, ariſes from the different ſtates in which we are when we touch them: if a very large pile be touched with dry hands, we experience the ſame ſenſation, as if we had touched a charged jar: on the contrary, if with hands thoroughly wetted and armed with metal we touch a jar previouſly diſcharged by dry hands, we receive a ſhock ſimilar to that of the galvanic pile.

Ritter reduces all the effects of the pile on the animal body to expanſions and contractions. By the poſitive conductor he has made ſeveral parts of the human body aſſume a greater bulk; and by the negative he has made the ſame parts contract. When the tongue is brought into contact with the poſitive conductor, applying the negative to ſome other part of the body, and they be all left a few minutes in this ſtate, a little riſing is produced on the tongue: the negative conductor, placed in contact with it in the ſame manner, produces a little

In the latter caſe the black oxide arranges itſelf in ſtars, as when electrified by the machine poſitively; and the circular figures of the former reſemble thoſe produced by electrifying the powder negatively.

When the poles are not made to communicate by means of a conductor, the chemical action on the ſtrata is very unequal. The plates of zinc are oxided leſs in proportion to their diſtance from the poſitive pole; and thoſe nearer the negative pole appear to have been protected from this action.

If every fifth pair be in contact with an iron wire communicating with water, the oxidation of the wires will be in the inverſe ratio of their diſtance from the poſitive pole; the central wire will not be more oxided, than one ſimply ſtanding in water, and the wires toward the negative pole will be preſerved freer from oxidation.

3. & 4. Spark and ſhock.

The action of the pile on the human body has been leaſt examined.

depreſſion. If the wet hands be kept a few minutes in contact with the poles of the pile, the pulſe of the hand in contact with the poſitive pole becomes ſenſibly ſtronger, and that of the hand which touches the negative pole weaker. Mr. Ritter has given us many particulars on this ſubject, and is employed in making additions to them; but we ſhall content ourſelves with obſerving, that the expanſion is followed by a ſenſation of heat, the contraction by a ſenſation of cold.

The action of the pile on the organs of ſenſe is modified by the particular nature of each; but it is remarkable, that the two poles of the pile produce in ſome ſort the two extremes of each ſpecies of ſenſation. I have already obſerved, in the abſtract I gave of Mr. Ritter's diſcoveries on light ſome time ago, that the pile produces in the eyes thoſe red and blue colours, which are nearly the extremes of thoſe of the priſm; and if it were not ſo difficult to diſtinguiſh the violet from the blue, undoubtedly we ſhould have nothing to wiſh in this reſpect. In theſe experiments, the eye in the poſitive ſtate, while it ſees every object of a red colour, ſees them at the ſame time larger and more diſtinctly: in the negative ſtate, on the contrary, it ſees them at once blue, ſmaller, and leſs diſtinct than they uſually appear.

Thus the expanſive power of the poſitive pole, and the contracting power of the negative, ſeem to exert their action here likewiſe.

The tongue is equally affected by the pile: the acid taſte produced by the poſitive conductor, and the alkaline by the negative are ſufficiently known.

The effect of the negative conductor on the noſe is an ammoniacal ſmell; that of the poſitive is a depreſſion of the ſenſibility of the organ, ſimilar to what is produced by the oxigenated muriatic acid.

The ears, touched by the poſitive conductor, hear a grave ſound, and with the negative, a ſound more acute.

Theſe experiments require much care: to repeat them properly it is neceſſary to read the deſcriptions at large, which the author has given in different tracts, where the particulars are minutely detailed.

It is in fact moſt difficult, and Ritter has brought on himſelf long and dangerous fits of illneſs by his experiments.

The ſkin, to form a good conductor, muſt be wetted; and this to a conſiderable extent, to have all the effect of a pile.

If the ſkin be wetted and armed with metal in two places, and touched with conductors from the pile, the ſenſation on the ſmaller ſurface is moſt perceptible, and often painful: hence we can command the magnitude of the effect we would produce on the body.

The difference between the ſhock from the pile, and that from the jar, ariſes from the ſtate in which we are when we touch them. If we touch a large pile with dry hands, we receive a ſmart ſhock; but if with hands wetted and armed with metal, we touch a jar diſcharged previouſly by dry hands, we feel the ſame ſenſation as from the pile. Ritter reduces all the effect of the pile on the body to expanſion and contraction; the poſitive conductor expanding, the negative contracting the part. The pulſe is made ſtronger by the poſitive conductor; weaker by the negative. The expanſion is followed by a ſenſation of heat, and *vice verſa*. The action of the pile on the organs of ſenſe depends on the nature of the organ; but the two poles produce the extremes of the ſenſation. The eye in the poſitive ſtate ſees objects red, large, and diſtinct; in the negative, blue, ſmall, and more obſcure. On the tongue the poſitive pole produces an acid taſte; the negative, an alkaline. In the noſe the negative produces an alkaline ſmell; the poſitive, that of oxigenated muriatic acid. In the ears the poſitive produces a grave ſound; the negative, an acute.

I X.

Account of a Machine for laying Land level.

By Mr. DAVID CHARLES [*].

THIS ſimple machine, which is the invention of my Steward, and of which I have ſeen nothing ſimilar, appears to me neceſſary, even in the moſt fertile parts of England, where the new ſyſtem of drill-huſbandry has been introduced, or even where there is any attention to the waſte of time, or to the eaſe of cattle in the act of ploughing; in order to get rid of crooked or unequal ridges, without either a ſummer fallow by croſs ploughing, or elſe by frequent repetitions of ploughing in the winter and ſpring, which the humidity of our climate will not allow in every kind of ſoil.

Account of a machine for laying land level.

I reduced fourteen acres of land laſt ſpring to a perfect level, where the crowns of the ridges were above two feet higher than the furrows, and where they were crooked and of unequal breadths. Six acres of this is now under turnips, a crop that gives ſufficient time to ameliorate the under-ſtrata of ſoil that had perhaps never before been expoſed to the influence of the ſun and air; and by the adoption of the Northumberland mode of ſowing that root on dunged drills, it is almoſt immaterial where the upper ſtrata is, provided the ſeed vegetates, as it ſoon ſtrikes into the manure, and rapidly flouriſhes.

My chief ſucceſs, however, has been upon a field of eight acres, which lay in the unprofitable ſtate already deſcribed. This land, which is a deep clay, and which had produced a crop of wheat from an old lay-ſod the former year without any manure, was winter ploughed, and lay in that ſtate until the leveller was introduced the firſt dry weather in April. It was preceded by two horſe-ploughs, taking perhaps a ſquare of an acre at once: theſe looſened the ſoil the depth of a common furrow, and twice the breadth acroſs the ridges. The leveller followed, drawn by two oxen and two horſes, with a man at each handle, to preſs it down where the weight is to be removed, and to lift up the body by the handles

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[*] From the Tranſactions of the Society of Arts, who rewarded the Inventor with their ſilver medal. The communication was made by Lieut. Col. Hardy, of Weſtmead, Carmarthenſhire.

where

Account of a machine for laying land level.

where it is to be diſcharged. Thus four men, one driver, and eight head of cattle, will more effectually level from half an acre to three roods in one day, according as the earth is light or heavy, than ſixty or eighty men would accompliſh with barrows and ſhovels, &c. even with the aſſiſtance of a plough. In ſandy ground where the depth of one furrow will bring all to a level, as much will of courſe be done in one day as two ploughs can cover; but my ground required to be gone over ſeveral times. After this field was levelled, the backs of the ridges, as they are termed, which were ſtripped of their vegetable mold, were ploughed up, the furrows not requiring it. They were alſo harrowed, and the field copiouſly manured with lime compoſt; harrowed in, and broke into nine-feet ridges, perfectly ſtrait, in order to introduce Duckit's drill. It was ſown under furrow, broadcaſt, the laſt of it not until the 13th of May, and was cut down a reaſonable crop the 4th of September. I am now thraſhing it, and a ſample ſhall be ſent, as well as a return of the eight acres if neceſſary.

The field now lies in proper form, well manured, with the advantage of a fair crop from heavy tenacious ground, without loſing a ſeaſon, and in a year by no means favourable.

I am well aware there are many ſhallow ſoils, where it may be hazardous to remove the enriched ſurface, and truſt perhaps one half of your land for a crop that had never before been expoſed to the atmoſphere; but where the ſoil is ſufficiently deep, or you have good under-ſtrata, and there is manure at hand to correct what is ſour from want of expoſure and tillage, it is evident from this experiment that no riſk is run.

To avoid the expence of a fallow, and to lay out ground in ſtrait and even ridges, even where drill huſbandry is not practiſed, ſhould be objects to every rational farmer. But where the new ſyſtem is intended to be adopted, it becomes indiſpenſably neceſſary. In laying down lawns, parks, &c. where furrows are an eyeſore, or places inacceſſible to wheel carriages from their declivity, and from which earth is to be removed, it will be found equally uſeful.

Should the ſociety conſider the inventor, David Charles, worthy of any renumeration, honorary or otherwiſe, it will be gratefully acknowledged by

Your obedient Servant,

Weſtmead, Jan. 1, 1803. JOSEPH HARDY.

Account of a machine for laying land level.

Certificates from Mr. Owen Edwards, of Brook, and Thomas Bynan, carpenter, of Weſtmead, accompanied the above letter, confirming the ſtatement made herein.

Deſcription of the Machine for laying Uneven Land level, invented by Mr. DAVID CHARLES.—*Plate* XI. Fig. 1, 2.

Fig. 1.—A, Part of the pole to which the oxen or horſes which draw the machine are faſtened, and which is attached to the machine by a pin at B.

CC, The two wheels, ſhod with iron, which run upon the axle D.

EE, The upper frame-work of the machine, extending from the axle to the extremity of the handles FF, and ſecured firmly by the croſs pieces.

GG, The curved iron ſliders of the machine, which may be raiſed or depreſſed a little by means of the pins HH, which paſs through holes in the wood-work, and alſo in the iron ſliders; theſe ſliders form one piece with the back iron ſcraper I, in the manner more fully explained in *Fig.* 2.

K, The wooden back of the machine, which ſhould be made ſtrong, to reſiſt the weight of the earth when collected therein. The iron ſcraper ſhould be firmly ſecured to this by ſcrews and iron-work.

LL, The wooden ſides of the machine firmly connected with the back and frame-work, in order to aſſiſt in collecting the earth to be removed.

M, A ſtrong croſs piece into which the ribs which ſupport the back are well morticed.

Fig. 2.—K, The interior part of the back of the machine.

I, The iron ſcraper, ſharp at the bottom, and firmly ſcrewed to the back of the machine.

GG, Parts of the ſide irons or ſliders, ſhowing the mode in which they are united with the ſcraper I.

M, The croſs piece above deſcribed.

Experiments

X.

Experiments on Magnetiſm; by Mr. RITTER, *of Jena. Communicated by Dr.* ORSTED, *of Copenhagen**.

The phenomena of magnetiſm and electricity have often been compared, but we want facts.

THE phenomena of magnetiſm have frequently been compared with thoſe of electricity, and many facts ſeem to juſtiſy the compariſon. Theſe facts, however, are neither numerous enough, nor ſufficiently concluſive, to compoſe a complete theory. A ſeries of experiments, exhibiting the magnetic needle in all its relations to electricity, at preſent better known by means of the pile, would undoubtedly throw much light on a ſubject heretofore ſo obſcure. Ritter felt the importance of ſuch an undertaking, and began freſh inquiries concerning magnetiſm, with the ſame ardour and ſagacity that have ever diſtinguiſhed his labours. Though theſe experiments did not always anſwer the full extent of his deſigns, they notwithſtanding exhibit a ſufficient number of intereſting facts to excite the curioſity of every natural philoſopher.

Ritter has inſtituted new inquiries on the ſubject.

His experiments did not always anſwer, but exhibited many intereſting facts.

A magnetic wire, and another not magnetic, excited galvanic palpitations in frogs; the ſouth pole more ſtrongly, the north leſs, than iron not magnetic.

Hence he inferred, that the ſouth pole has a greater affinity for oxigen; the north, leſs.

Mr. Ritter's firſt experiments with the magnet were on frogs. He found that a magnetic iron wire, with another not magnetic, excited a galvanic palpitation in theſe animals. Preſently he obſerved, that the ſouth pole excited ſtronger palpitations, and the north pole weaker, than the iron not magnetic. Having conſtantly noticed, that the metals moſt ſuſceptible of oxidation excited the ſtrongeſt palpitations, he inferred, that the ſouth pole poſſeſſes a greater affinity for oxigen than ſimple iron, and the north pole leſs.

This confirmed by experiment.

Magnetic iron wires being placed in weak nitric acid, the ſouth pole was moſt oxided.

This ſuppoſition he confirmed by means of ſeveral chemical re-agents. He placed a magnetic iron wire on pieces of glaſs in a plate of earthen ware, and poured upon it very weak nitric acid. The ſouth pole was attacked by the acid much more powerfully than the north; and was ſoon ſurrounded by a depoſition of oxigen, the quantity of which greatly exceeded that of the other pole.

The ſouth pole of a magnetic wire being immerſed in one phial of water,

The different oxidability of the magnetic poles is very well exhibited likewiſe, by taking three ſmall bottles of equal ſize, filled with water, either pure or ſlightly acidulated, and putting into one the ſouth polar end of a magnetic wire, into a ſecond the north polar end of a ſimilar wire, and into the third the end of an equal wire not magnetic: the ſouth pole will firſt begin to depoſit oxide, the unmagnetic iron a little after, and the north pole laſt. This experiment requires conſiderable care. The ſurface of the water muſt be covered with very freſh oil of almonds, to exclude all acceſs of air. Care muſt be taken too, that one of the bottles is not more expoſed to the ſun than the others, becauſe light accelerates oxidation. Ritter convinced himſelf of this by direct experiments; expoſing two iron wires in water to the ſun, but covering one of the phials with black paper, when that in the phial left uncovered was oxided much the more quickly.

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* Journal de Phyſique, December, 1803, Vol. LVII. p. 406.

the north pole of one in another, and the end of a ſimple wire in a third; the ſouth pole firſt depoſits oxide, and the north pole laſt. This experiment requires particular precautions. Air muſt be excluded; and light accelerates oxidation.

If infuſion of litmus be ſubſtituted inſtead of the water in the three phials in the preceding experiment, the relative oxidations will be the ſame; but they will be attended with a change of colour, ſhewing, that an acid is produced proportional to each oxidation; ſo that the ſouth pole not only undergoes the greateſt oxidation, but likewiſe reddens the infuſion of litmus moſt *. The action that takes place in this experiment is very feeble, and frequently requires a week or more to produce a diſtinct effect; and indeed to accelerate it ſo much as this, it is neceſſary to add, previouſly to the infuſion, as much acetic acid as will incline it to red, without completely changing its colour. The infuſion reddened in this experiment reſumes its blue colour on expoſure to the air: but we muſt not hence conclude, that the acid produced by the action of the magnet is very volatile, for infuſion of litmus reddened by phoſphoric acid, or any other, exhibits the ſame phenomenon.

If infuſion of litmus be uſed, the relative oxidations will be the ſame; but an acid in proportion to each will be produced, as appears by the change of colour. This action is very ſlow; and to accelerate it a ſmall portion of acetic acid ſhould be added to the infuſion. The infuſion thus reddened becomes blue again on expoſure to the air: but it is the ſame when reddened with any other acid.

The following experiment exhibits ſome things peculiar, and therefore I ſhall give it more at large. It has not been repeated, but the harmony of its reſults is in favour of its accuracy. Sixteen magnetic wires, of equal ſize and power, were placed in ſix veſſels, all equally full of a mixture of one part nitric acid and thirty-ſix parts water, in the following

Experiment.

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* Ritter has remarked, that the oxidation of zinc, and ſeveral other metals, in pure water, produces an acid. Mr. Jager, a celebrated phyſician of Stuttgard, made the ſame diſcovery, without knowing any thing of Ritter's.

Ritter and Jager have found, that the oxidation of ſeveral metals in pure water produces an acid.

In phials containing water 36 parts, nitric acid 1 part, were immerſed: 1. a N. and S. pole, at ½ a line diſtance: 2. the ſame 1¾ inch: 3. 3 S. poles, at ½ a line: 4. the ſame at 1¾ inch: 5. 3 N. poles, at ½ a line: 6 the ſame at 1¾.

The oxide depoſited in theſe with reſpect to quantity was in the following order: No. 2, 1, 3, 4, 5, 6.

The loſs of fluid by evaporation, was in the inverſe ratio of the oxidations.

All the magnets were weakened in power: No. 2 leaſt; 1 more: in all the reſt one wire was weakened more than the others.

manner: In the firſt glaſs were placed two wires, one with the north pole immerſed in the fluid, the other with the ſouth, and not more than half a line aſunder: In the ſecond, the ſame, but the wires an inch and three quarters apart: In the third and fourth were each three wires, with the ſouth poles of all immerſed, but their diſtances in the two glaſſes different, as in the firſt and ſecond: In the fifth and ſixth were wires ſimilarly arranged, but with the north poles immerſed. Different quantities of oxide were gradually depoſed; and to expreſs the whole in few words, we will call the ſouth pole S, the north pole N, their greater diſtance g, and their leſs p; and we will expreſs the order of oxidations as follows: $S N g \succ S N p \succ 3 S p \succ 3 S g \succ 3 N p \succ 3 N g \succ$. On the nineteenth day it was obſerved, that the loſs of fluid by evaporation had not been equal in all the veſſels, but took place in the inverſe order of the oxidations. All the magnetic wires were weakened in power; N S g leaſt; N S p more: of the wires 3 S p, two had loſt leſs power than the third; and in like manner 3 S g, 3 N p, 3 N g, had each two left more powerful than the third; the ſtrongeſt were equal to N S g.

On immerſing two ſouth poles in one phial, two north in another: the latter were moſt oxided.

In another experiment, where two little veſſels filled with infuſion of litmus were employed, one of them containing two magnetic wires, the ſouth poles of which were immerſed in the fluid; the other two ſimilar wires, of which the oppoſite poles were immerſed; the oxidation was greateſt in the latter veſſel*.

Mr. R. attempted unſucceſsfully to conſtruct a battery with 120 magnetic wires, their poles placed in oppoſition, and ſeparated by a drop of water; but he has not given up the deſign: and means to purſue his inquiries on the ſubject at large very extenſively.

Laſtly, Mr. Ritter endeavoured to conſtruct a battery of magnets, but he did not ſucceed. For this purpoſe he employed a hundred and twenty magnetic wires, placed ſo that each pole had its contrary oppoſite to it, and ſeparated from it by a drop of water; but this apparatus produced no effect. The ingenious author, however, has not relinquiſhed the hope of being able to compoſe a magnetic battery, though other experiments, not leſs important, have hitherto prevented him. This ſeries of experiments he conſiders only as the commencement of a very extenſive labour, the reſults of which we hope ſoon to obtain.

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* This appears contradictory to the experiment adduced in the third paragraph. T.

XI.

Obſervations on Arſeniated Copper. *By* HAUY [*].

Hiſtory of the diſcovery of the ores, and their compoſition.

THE only ores of arſeniated copper which are well known, are thoſe from the county of Cornwall in England. The determination of their true compoſition followed cloſely on the diſcovery of this metallic ſubſtance, for which we are indebted to the fortunate chance which threw ſome ſpecimens into the hands of the celebrated Klaproth. It was in 1787 that he publiſhed in the *Journal de la Société des Curieux de la Nature*[†], the reſult of the examination which he had made of this new mineral.

The authors who have ſpoken of arſeniated copper ſince that period, had only deſcribed it under the form of acicular cryſtals, when Citizen Lelievre, member of the Council of Mines, having ſuſpected the exiſtence of a peculiar ſubſtance, from the inſpection of a group of green hexagonal bevilled laminæ which were given to him, made an eſſay of it, and diſcovered the preſence of oxide of copper and arſenic acid. Citizen Vauquelin ſoon afterwards confirmed this indication, and determined the proportion of the relative quantities of the two principles contained in the ſame ſubſtance.

About this time the opening of a ſecond mine in the county of Cornwall occaſioned the re-appearance of arſeniated copper, the vein which had been formerly explored being exhauſted. This diſcovery was the more important, as the ſubſtance appeared, in its new ſituation, with characters altogether peculiar, and under forms hitherto unknown.

M. de Bournon, who was at hand to participate in this increaſe of riches which reſulted to mineralogy, ſent to Citizen Gillet Laumont and me, ſeveral ſpecimens choſen from among thoſe he poſſeſſed; and that which added to the value of his [illegible] was his haſte to communicate to us the intereſting work which he had prepared, on the cryſtallography of arſeniated copper, before he publiſhed it.

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[*] Tranſlated from a pamphlet in quarto, ſent by the author to the Count de Bournon; probably forming part of a journal.

[†] Tom. VIII. p. 160.

Mr. Chenevix was employed at the ſame time on the analyſis of this ſubſtance. Soon after they both publiſhed the reſults of their reſearches in the *Philoſophical Tranſactions* *; and Mr. Chenevix teſtifies his admiration at ſuch a perfect agreement between two ſciences which employ two methods ſo different to interrogate nature. M. de Bournon, on his part, ſays that the analyſes of Mr. Chenevix have given the moſt ſatisfactory ſanction to the diviſion which he had himſelf made, of the arſeniated copper into four diſtinct ſpecies.

Deſcription of the varieties.

Before going farther, it is neceſſary to make known the varieties of arſeniated copper which I have been enabled to examine. I ſhall confine myſelf to giving a deſcription of them, ſuccinct and independent of the laws to which the ſtructure of cryſtals is ſubmitted, the actual ſtate of our knowledge on this ſubject only admitting of hypothetical views, of which I ſhall ſpeak hereafter.

1. Obtuſe octahedral arſeniated copper, (*Plate* IX. *Fig.* 2): incidence of P on p, 50° 4′; of P' on p', 65° 8′; of P on P', 139° 47′ †. The colour of the cryſtals is ſometimes a fine celeſtial blue, and ſometimes a green, which varies between a graſs-green and a pale green. The octahedron ſometimes becomes cuneiform, lengthening ſo that the terminal edge is parallel at D.

2. Lamelliform arſeniated copper. In hexagonal laminæ, whoſe narrow faces are inclined alternately in contrary directions; the incidence of two of the narrow faces, ſituated on the ſame ſide, on the correſpondent baſe, 135° nearly, according to M. de Bournon: incidence of the third on the ſame baſe, 115° nearly.

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* For 1801, p. 199, *et ſeq:*

† I adopt here, very nearly, the reſults of M. de Bournon, who indicates 50° for the incidence of P on p, and 65° for that of P' on p'. I have only endeavoured to find limits capable of facilitating the calculations which I propoſe to make. Let bac, gac, (*Fig.* 4) be the ſame faces as P and P', (*Fig.* 2); let ao (*Fig.* 4) be the height of the pyramid which has its ſummit in A, (*Fig.* 2); on (*Fig.* 4) a perpendicular to bc, and or perpendicular to cg:—if $ao = \sqrt{588}$, $on = \sqrt{2695}$, and or $\sqrt{1440}$, we ſhall have 50° 4′ for the incidence of P on p (*Fig.* 2), and 65° 8′ for that of P' on p': whence is deduced, by calculation, 139° 47′ for that of P on P'.

The laminæ divide parallel to the large faces with great eaſe. Their colour is a fine graſs-green.

3. Acute octahedral arſeniated copper, (*Fig.* 3): Incidence of *r* on *r'*, 96°, according to M. de Bournon; of *l* on *l'*, 112°. The colour is a brown green more or leſs deep.

a, cuneiform. The preceding octahædron lengthened ſo that the terminal edge is parallel to *n*. This form, which is the moſt common, offers the appearance of a long rhomboidal priſm, more or leſs acute, and is terminated by dihedral ſummits.

4. Trihedral arſeniated copper. In a ſtrait triangular priſm, which is at the ſame time equilateral, according to M. de Bournon.

When the cryſtals have not been long expoſed, their colour is a fine bluiſh green; but their ſurface is ſubject to change and take a blackiſh tinge. If they are ſcratched, their primitive colour will re-appear.

5. Capillary arſeniated copper. This is properly the olivenerz of the German mineralogiſts.

6. Mammellated arſeniated copper. In mammellated maſſes, ſtriated in the interior. Theſe two laſt varieties are ſuſceptible of a great diverſity of tints, which ſhew the tranſitions from graſs-green to olive-green, to greeniſh-brown, to tawny (*mordoré*), to yellow, to bluiſh, and to white, which is frequently ſattiny.

The following is the manner in which M. de Bournon claſſed the different modifications which have been juſt mentioned, according to the differences which they offer with reſpect to form, ſpecific gravity and hardneſs.

M. de Bournon's claſſification.

He divides them, as I have ſaid, into four diſtinct ſpecies. The firſt is derived from the obtuſe octahedron: the type of the ſecond is the lamelliform cryſtal, in hexagonal laminæ, with bevils inclined alternately in contrary directions. He takes the acute octahedron for the primitive form of the third, and connects the acicular cryſtals and mammelary concretions with it, as varieties: in the fourth he places the equilateral triangular priſm, and ſeveral other forms which offer the ſame priſm, truncated on its ſolid angles or on its edges.

Four diſtinct ſpecies.

On the other hand, Mr. Chenevix has given ſix reſults of the analyſis of arſeniated copper, which I ſhall detail, diſpoſing them conformably to the order eſtabliſhed by M. de Bournon.

Mr. Chenevix's analyſes.

Firſt Species, in obtuſe octahedra.

Oxide of copper	49
Arſenic acid	14
Water	35
Loſs	2
	100

Second Species, in lamelliform Cryſtals.

Oxide of copper	58
Arſenic acid	21
Water	21
	100

Third Species, in acute octahedra.

Oxide of copper	60
Arſenic acid	39.7
Loſs	0.3
	100.0

Variety of the ſame Species, in capillary Cryſtals.

Oxide of copper	51
Arſenic acid	29
Water	18
Loſs	2
	100

Another Variety, in mammellated Concretions.

Oxide of copper	50
Arſenic acid	29
Water	21
	100

Fourth Species, in trihedral Priſms.

Oxide of copper	54
Arſenic acid	30
Water	16
	100

Towards

Only one true arſeniate of copper: the others are arſeniates of hidrate of copper.

Towards the concluſion of his memoir, Mr. Chenevix remarks that the natural arſeniate of copper exiſts in three different combinations, the firſt of which contains 11 *per cent.* of arſenic acid, (firſt reſult above); the ſecond contains 21, (ſecond reſult), and the third [illegible], (third, fourth, fifth, and ſixth reſults). It is true, the third reſult gave 39.7 of acid in the 100 parts, but as the remainder of the maſs was compoſed of 60 parts of copper without water, he found that the proportion of the acid with the copper did not differ much from that which takes place in the varieties in which water forms a part. This induced Mr. Chenevix to comprehend this reſult in the ſame diviſion. Yet he conſiders this combination as the only true arſeniate of copper, while the other three are arſeniates of hidrate of copper.

I feel the value of the double work from which I have given this extract ſo much the more, becauſe, having read the memoirs which contain the developement, I am enabled to judge of the advancement which it has produced in our knowledge on a ſubject which was in a great meaſure new when MM. de Bournon and Chenevix began to be employed with it. The expoſition which I ſhall add of ſome enquiries I have made on the cryſtallization of arſeniated copper, and of the reflections which they have given riſe to, have no other object but that nothing may be neglected which tends to elucidate in a greater degree every thing connected with an object of ſuch importance as the diſtinction of mineralogical ſpecies.

Can the varieties be reduced to one primitive form?

After having read the cryſtallographic part of the work in queſtion, I was deſirous to know if it was not poſſible to bring ſome of the cryſtals, deſcribed by M. de Bournon as belonging to different ſpecies, to the ſame form of the integrant molecule; but not being able to make all the direct obſervations which would have guided me in this enquiry, I was obliged to confine myſelf to ſimple hypotheſes.

I therefore conſidered the obtuſe octahedron as performing the functions of the primitive form; and I was the more warranted in conceiving this opinion, becauſe the celebrated Karſten, in a ſupplement to the excellent memoir which he had publiſhed before *, on the combinations of copper with different principles, ſays that the octahedron in queſtion is lamellated,

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* *Journ. de Phyſique*, Brumaire, An. X, p. [illegible], et ſuiv.

Compariſon of the acute and obtuſe octahedra.

in a direction parallel to the faces of the two pyramids, of which it is the aggregate*. Proceeding on this datum, I was curious to know if it was not poſſible to connect with the form of the obtuſe octahedron in queſtion, that of the acute octahedron, which M. de Bournon has taken for the type of his third ſpecies. Let *P*, *P′* (*Fig.* 2) be ſtill an obtuſe octahedron, in which the incidence of *P* on *p* is reckoned to be 50° 4′, and that of *P′* on *p′*, 65° 8′, conformably to the meaſures indicated above: if we imagine another octahedron (*Fig.* 3) the ſign of which is $\underset{l}{\underset{2}{\overset{2}{\mathrm{D}}}}\,\underset{r}{\underset{4}{\overset{4}{\mathrm{F}}}}$, we ſhall find that the incidence of *l* on *l′* is 109°, and that of *r* on *r′* is 93° 36′. Now the correſpondent incidences determined by M. de Bournon, are one 112°, and the other 96°; which in one caſe makes a difference of 3°, and in the other of 2° 24′.

The differences in the angles accounted for.

If the meaſures had been taken on cryſtals ſo well defined, that the differences could be conſidered as real, we muſt have concluded that they formed two diſtinct ſpecies, becauſe even theſe differences could only have been done away by ſuppoſing the laws of decrement to be much too complicated to be admiſſible.

But if the cryſtals were not capable of very accurate meaſurement, we can the better conceive that the differences were ſimply apparent, and that it may be poſſible that the error did not wholly ariſe in one obſervation, ſince it was neceſſary to make two, in which ſmall deviations might have been produced on oppoſite ſides; and then mechanical diviſion alone, by giving different reſults with reſpect to the two octahedra, would have ſhown that a conformity between the angles obſerved and thoſe meaſured, would be purely accidental.

Compariſon of the lamelliform variety with the obtuſe octahedron.

I afterwards compared the lamelliform variety with alternate bevils, which is the ſecond ſpecies of M. de Bournon, with the ſame octahedron with obtuſe ſummits. Now, if we ſuppoſe two interſecting planes, parallel to the face P′, and which meet the center, they will detach an octahedral ſegment which cannot be ſuppoſed to have much thickneſs, and whoſe two large faces will be hexagons, and the ſix lateral faces, trapeziums, inclined to the great faces *. But theſe trapeziums will not be ſituated alternately in oppoſite directions. The three which will form obtuſe angles with the large faces, will be contiguous to each other, and the ſame will be the caſe with thoſe which form acute angles with the ſame faces. For example, thoſe of the trapeziums which form obtuſe angles with the large face analogous to *P′*, will correſpond to the two faces of the adjacent octahedra at *B*, *B′*, and to the face ſituated behind *A*, parallel to *P*. The inclination of this latter face on *P* is, according to M. de Bournon, 115°; and the two others, as I have indicated them above according to my calculations, are each about 139½°.

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* Journ. de Phyſique, Pluvioſe, An. X. p. 151.

Now, of the three lateral trapeziums in the lamelliform arſenical copper, one has the ſame inclination of 115° on this baſe, according to M. de Bournon, and the two others have 135°; an eſtimate which he only gives as an approximation, and which only differs by 4½° from that correſponding to it in the obtuſe octahedron, (*Fig.* 2).

The great difference conſiſts in this, that the three lateral trapeziums which look towards the ſame baſe, in the octahedral ſegment I have deſcribed, are contiguous to each other, as I have ſaid; while thoſe of the lamelliform arſeniated copper alternate with the three others which look towards the oppoſite baſe †.

But there is a method of removing this difficulty. Let us conceive that the two ſections made in the octahedron (*Fig.* 2), inſtead of being parallel to the face *P′*, are ſo to the face *P*. In this caſe the lateral trapeziums, ſituated on the two ſides of the edges *B*, *B′*, will always have an inclination of 139½° to the ſuperior baſe. Now, if the ſegment parallel to *p* makes an angle of 115° with the baſe analogous to *P*, the three ſegments will preſerve, with reſpect to thoſe turned towards the oppoſite baſe, the alternation indicated by M. de Bournon.

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* Several ſubſtances, among others the ſpinelle, offer examples of ſimilar ſegments.

† The figure given by M. de Bournon, of which that is a copy in *Plate XI. Fig.* 5, ſeems to have been traced according to the condition that the three trapeziums turned towards the ſame baſe ſhould be contiguous. This was doubtleſs an overſight of the draughtſman.

But the incidence of p on P gives, on the contrary, an acute angle of 50°. Now, let us imagine a decrement indicated by $_{,}D$, which acts on the face p and on that which is oppoſite; the faces produced will be ſituated vertically; whence it follows that that which will mark the face p will form an angle with P equal to 90° *plùs* 25°, which is the half of the inclination of p to P, that is to ſay, the angle in queſtion will be 115°, conformably to obſervation *.

Their diviſion into four ſpecies is not admitted by the laws of ſtructure.

I ſhall not urge theſe reſults farther, which, as I have already ſtated, I only offer as purely hypothetical; and I ſhall abſtain from adding my opinion with reſpect to the fourth of the ſpecies admitted by M. de Bournon, which, according to him, has the equilateral triangular priſm for a primitive form. It is enough for me to have ſhown that, with reſpect to the diviſion of arſeniated copper into four diſtinct ſpecies, the laws of the ſtructure may give riſe to doubts which deſerve ſome attention. If they can be removed, as it is not impoſſible they may, another proof will be obtained in favour of an opinion on which no obſcurity ſhould remain, that it may be worthy of being unanimouſly adopted.

Compariſon of Mr. Chenevix's analyſes and M. de Bournon's diviſion.

If we now conſider the reſults of the analyſes which Mr. Chenevix has made of the different modifications of arſeniated copper, we find, that in ſuch of theſe analyſes as have had for their object the types of the four ſpecies admitted by M. de Bournon, three have given ſenſible differences in the relative quantities of copper, arſenic acid, and water. Theſe analyſes correſpond with the firſt, third, and fourth ſpecies. Another analyſis, made on the ſecond ſpecies, gave only copper and arſenic acid, without water. Thus, ſuppoſing that the relations between the quantities of the three principles contained in the modifications which Mr. Chenevix calls *arſeniates of hidrate of copper*, conſtitute true limits, and that, in the modification which he calls ſimply *arſeniate of copper*, the abſence of water depends on the nature of the ſubſtance itſelf; we ſhall, in this reſpect, find an agreement between the reſults of analyſis and thoſe of cryſtallography, very favourable to the ſubdiviſion of the mineral in queſtion, into four diſtinct ſpecies.

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* I have a lamelliform cryſtal, on which, inſtead of a ſimple bevil, there are two, ſituated in contrary directions on the ſides of the ſame edge; but they are too ſmall for it to be poſſible to determine the poſitions exactly.

Vauquelin's analyſis of the lamelliform arſeniate:

But Cit. Vauquelin, on analyſing a piece of lamelliform arſeniated copper, whoſe cryſtals were quite freſh, obtained a very different relation between the quantities of the three principles*. His reſult was as follows:

Oxide of copper - - -	39
Arſenic acid - - - -	43
Water - - - -	17
Loſs - - - -	1
	100

It is remarkable in this caſe, that the quantity of acid exceeds that of the copper, while in the reſult obtained by Mr. Chenevix it forms only a little more than a third of the quantity of copper. It is not therefore evident that the limits indicated by this celebrated chemiſt are eſſential to the ſubſtances analyſed.

and of the capillary cryſtals.

The experiments of the ſame philoſophers on the capillary cryſtals, offer differences not leſs ſtriking. According to Cit. Vauquelin theſe cryſtals contain,

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* Mr. Chenevix, in his memoir, gives a paſſage from a letter which Citizen Vauquelin had written to him, and in which he informed him, that having analyzed cryſtals of the lamelliform variety, he found that they were compoſed of 59 of oxide of copper and 41 of arſenic acid. Mr. Chenevix adds, that the great difference between this reſult and that which he had himſelf obtained from the ſame ſubſtance, induced him to repeat his analyſis with great care and attention, and that he conſtantly found the ſame proportions of oxide of copper, arſenic, and water. It is very probable that this reſult announced by Cit. Vauquelin, and ſo different on the other hand from that now given, was obtained in a firſt eſſay, or that this chemiſt, when he wrote to Mr. Chenevix, truſted to his memory, which was not ſo faithful as it generally is. However that may have been, the only reſult avowed by Cit. Vauquelin is that we now publiſh, and which he has inſerted in the *Journal des Mines*, No. 55, p. 562.

Silex	2
Water	5
Arſeniate of iron	7 to 8
Arſeniate of copper	86
	100

This chemiſt adds, that if the arſeniate of copper did not contain any foreign matter, it would be formed of about 69 parts of oxide of copper and 31 of arſenic acid.

Klaproth's analyſis.

We have another reſult on the ſame ſubject, obtained by M. Klaproth, whoſe labours have concurred ſo advantageouſly with thoſe of Vauquelin, to procure an exact knowledge of the compoſition of minerals. His reſult gave,

Oxide of copper	50.62
Arſenic acid	45.00
Water	3.50
Loſs	88
	100.00 *

The quantity of copper is nearly the ſame as in Mr. Chenevix's reſult; but on one ſide we find 45 of acid with 3.5 of water, and on the other only 29 of acid and 18 of water; which is very different.

Mr. Chenevix's reſults do not agree with thoſe of M. de Bournon.

Beſides, we need only keep to Mr. Chenevix's own reſults to find difficulties and cauſes of uncertainty; for while this celebrated chemiſt obtained a very ſenſible quantity of water from the capillary cryſtals and the mammellated maſſes, theſe two modifications were conſidered by M. de Bournon as ſimple varieties of the third ſpecies, which is the acute octahedron, and which gave only copper and arſenic acid without water. Further, if the analyſes of the capillary cryſtals and of the mammellated maſſes, are compared with that of the cryſtals in trihedral priſms, it will be ſeen that the differences do not exceed thoſe which are frequently met with between the analyſes of ſeveral pieces which evidently belong to the ſame ſpecies of mineral.

I add, that M. de Bournon ſeems to have had more authority for conſidering the capillary cryſtals and the mammellated concretions as ſimple varieties of the acute octahedron, ſince he indicates the intermediate modifications which connect theſe varieties with their type; ſo that, according to him, there are cryſtals which are perfectly determined in one part of their length, and fibrous at their extremity.

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* *Additions à la Connoiſſance chimique des Minéraux*, p. 192.

A fifth ſpecies propoſed.

M. de Bournon, doubtleſs ſtruck with the exception which the agreement between the two ſciences, announced by Mr. Chenevix and himſelf, ſeemed liable to, in this inſtance, has ſince inſerted in Mr. Nicholſon's Journal* a note, in which he propoſes to eſtabliſh a fifth ſpecies of arſeniated copper, compoſed of the capillary cryſtals and mammeliated maſſes, which ſeems to operate leſs in removing the difficulty than in bringing it to light.

External differences ſhould not be admitted alone as evidence of a different ſpecies.

It cannot be denied that the modifications of arſeniated copper offer ſenſible differences in their aſpect, their exterior forms, and their colours. M. de Bournon alſo indicates ſome in their hardneſs and in their ſpecific gravities. But the reduction of natural beings to the ſmalleſt poſſible number of ſpecies, really diſtinct, is an advantage of ſuch value to ſcience, which it perfectionates by ſimplifying it, that, before ſeparating ſubſtances, according to thoſe diverſities which ſeem to be at variance with the relations which they otherwiſe have, and before ſeeking particular ſpecific names for them, which would be neceſſary, all the means of aſcertaining that the diverſities in queſtion are not purely accidental, ſhould be exhauſted. Even though the reſearches which ſtill remain to be made for the accompliſhment of this object, ſhould have no other effect but to cauſe the diſappearance of one ſingle ſpecies, admitted by the two celebrated men whoſe reſults I have ſet forth, from the ſyſtem, they will not be unprofitable to the progreſs of mineralogy.

⁂ A reply communicated by the Count de Bournon will appear in our next.

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* Philoſophical Journal, new Series. Vol. VII. p. 577.

XII.

Obſervations upon the Doctrine of Count Rumford reſpecting the want of direct conducting Power in Fluids with regard to Heat. By Cit. Berthollet.

(Concluded from page 140.)

Nicholſon, Thompſon and Murray have proved, that heat paſſes through the particles of fluids; that currents are often imaginary; and that fluids differ in conducting power.

I AM of opinion, that the experiments of Nicholſon, of Thompſon, and of Murray, leave no doubt on the communication of heat between the particles of liquids: ſome of them ſhow that the motions of the ſolid corpuſcules which are agitated in a liquid, may often miſlead, with reſpect to the currents which are believed to be perceptible: but their exiſtence muſt not, for this reaſon, be denied, when a difference between the ſpecific gravities is ſuddenly eſtabliſhed, and when the heat is communicated at the lower part of a veſſel. The others prove that the communication of heat may be made through a liquid in which no current can be ſuppoſed to tranſport it immediately to a ſolid body, and they prove that liquids are poſſeſſed of a conducting faculty which differs in its intenſity; but it is not to be inferred from this, that the locomotion of the particles of liquids does not contribute to eſtabliſh an equilibrium of temperature more ſpeedily: it is even probable, that the latter effect is generally the greateſt.

Theſe general facts account for all the phenomena.

The foregoing conſiderations, into which I have admitted the application of the faculty of communicating heat common to all bodies, of the conducting difference, and of the more ſpeedy diſtribution of heat by means of the difference of the ſpecific gravity which it introduces between the particles of a fluid, ſeem to me to account for all the phenomena which the diſcernment of Rumford has made public.

That gaſes receive heat very rapidly, is ſeen in the expanſion of air thermometers and air balloons.

Theſe conſiderations lead me to an opinion very different from his; it is known with what rapidity the thermoſcopes, or air thermometers, indicate the variations of temperature: Pictet could not obſerve a ſecond of difference between the elevation of a thermometer of this deſcription, and the emanation of radiant heat by a body placed at a diſtance: it has been obſerved, that aeroſtats experience a ſudden dilatation

tion by the appearance of the ſun*; theſe phenomena ſeem to me to indicate that the elaſtic fluids, far from being bad conductors, on the contrary, receive the temperature of other bodies very quickly; for, can it be ſuppoſed that all the particles of the gas take the temperature which they acquire by the contact of the covering of the balloon alone, and how can it be conceived, that the lower particles, which are contiguous to that portion of the covering which does not receive the ſolar emanations, ſhould be carried towards that which is expoſed to it? And ſince theſe particles at each contact only receive a part of the temperature to which they attain, what a prodigious whirlwind muſt there be ſuppoſed to be in the gas!

It appears to me, that the elaſtic fluids, inſtead of being bad conductors, poſſeſs this property in a high degree, although they probably differ from each other in this reſpect; and if air which is confined produces effects which ſeem to prove the contrary, they are owing to ſome circumſtance which modifies this property.

The ſlow conducting by confined gaſes aſcribed to ſome modification.

I think it is probable, that this circumſtance is the ſtate of compreſſion produced in a gas which cannot acquire a dilatation ſuitable to the temperature it receives; we have ſeen that caloric, in combining with the gaſes, only raiſes the temperature becauſe the dilatation meets with an obſtacle (107); hence it reſults, that the further the air is removed from the ſtate of dilatation, which it ſhould have, to be in equilibrium of temperature, the greater reſiſtance will it oppoſe to the combination of the caloric, and the more will it loſe of its conducting faculty, ſo that the air which would take the temperature of the ſurrounding bodies with facility, if it could acquire ſuitable dimenſions under a given preſſure, becomes a worſe and worſe conductor in proportion as it receives a temperature farther removed from the dimenſions which it can take. The air then experiences an effect, which may be compared to that of a body in which the force of coheſion obſtructs the action of a liquid, which can effect its ſolution as ſoon as this obſtruction begins to be diminiſhed.

The gas is probably prevented from acquiring heat, becauſe it cannot expand in that ſtate.

This explanation is applicable to the conſervative property of heat, which Rumford has proved to belong to the air which adheres to particles, ſuch as thoſe of the eider-down;

The effect of eider down, &c. is leſs to prevent the currents, than to oppoſe the ex-

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* Deſcrip. de l'aéroſtat de l'Acad. de Dijon.

this

panſion by its attraction, by which the air adheres to it.

this air only adheres by a true affinity, which probably reduces its dimenſions, or at leaſt, oppoſes its dilatation; and if the water can drive it off, it is only becauſe it combines with theſe ſubſtances, and adheres to their ſurface by its affinity; ſo that the air will then experience the ſame effect from the action of the affinity of the bodies to which it is adherent, as is produced on its elaſtic effort, by the ſpace within which it is confined, and in which it receives a higher temperature without having the power to dilate.

So that elaſtic fluids being more alterable in their volume, are alſo more diſpoſed to give and take heat.

Thus the elaſtic fluids which dilate much more by a ſimilar change of temperature than liquids and ſolids, muſt have the correſponding faculty of entering more eaſily into combination with caloric: they offer but little reſiſtance to compreſſion; they heat by the reduction of their volume; and they cool when they dilate: do not theſe effects announce a great diſpoſition to combine with caloric, or to abandon it, and to receive different degrees of ſaturation from it? and nevertheleſs, according to the opinion of Rumford, there muſt be an inſurmountable barrier between the moſt diſtant temperatures, of the different particles of a gas, when the particles do not meet with a ſolid body.

The ſame doctrine applied to liquid water: Sudden accumulation or abſorption by its fuſion, it conducts better than ice, &c.

It is poſſible, that liquid ſubſtances may be much better calculated to conduct heat than when they are in a ſolid ſtate; the properties of the reciprocal affinity which produces coheſion, ſeem to point this out: for ſince this affinity oppoſes the dilatation, it will offer an obſtacle to the combination of the caloric: this reſiſtance to its introduction is alſo proved by the quick accumulation which is made of it, as ſoon as the force of coheſion is deſtroyed, ſo that it is oppoſed to the combination of caloric, as well as to that of other ſubſtances; in fact, water ſeems to take the common temperature more eaſily, independent of the locomotion of its particles, than ice, which is a very bad conductor, and it is perhaps from this difference, that ice, and all the ſolids which paſs to the liquid ſtate, liquefy at the ſurface, inſtead of taking the common temperature.

I only offer theſe laſt explanations as conjectures, which may invite to experimental enquiries on a ſubject which is not indifferent to chemical theory.

XIII.

A Memoir on the Movements which certain Fluids receive from the Contact of other Fluids[*]. *By* J. DRAPARNAUD, *Curator and Profeſſor of Natural Hiſtory, at the Medicinal School of Montpelier.*

Experiment on the action of alcohol which led to the diſcovery of this impulſe.

I HAD obſerved that alcohol attacks, and finiſhes by even, at length, deſtroying the calcareous covering of the molluſcæ, which are put into it to be preſerved. Suppoſing that this might ariſe from the alcohol, particularly that which is not well rectified, containing a little acetous acid, I put a little tincture of turnſole into a glaſs capſule, and poured into it a a few drops of alcohol. The tincture did not change colour, but to my great ſurpriſe it moved towards the circumference with great vivacity, leaving the bottom of the capſule uncovered: when it had reached the maximum of diſperſion, it returned again, and covered the bottom of the veſſel which it had abandoned. It is evident, therefore, that miſſing what I ſought, I found that which I did not ſeek, which frequently occurs in the courſe of experiments.

Prevoſt's memoirs on the emanations of odorant bodies.

This curious experiment induced me to make a multitude of others, and to try a great number of ſubſtances. At the moment of putting theſe experiments in order, and of compoſing this memoir, I recollected having read that *M. Benedict Prevoſt* had produced this repulſion of water by means of volatile oils, and even of many ſolid odorant bodies. I therefore conſulted the two memoirs which this ingenious philoſopher has inſerted in the Annals, and whoſe ſubject is, *The methods of rendering the emanations of odorant bodies ſenſible to the ſight*[†]. Although my experiments were made with another view, I paſs over, in ſilence, all which are conformable to thoſe of *M. Prevoſt*, and which I made by employing the ſame ſubſtances: I ſhall only ſpeak of thoſe which differ from his, either in the reſults obtained; or in the means employed.

Action of alcohol on water.

1. If a thin ſtratum of water is put on the bottom of a veſſel, and a drop of alcohol is brought to the centre of this

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[*] From the Annales de Chimie. Fructidor, An. XI.

[†] Annales de Chimie. Tom. XXI. et XXIV.

ſtratum, with a glaſs rod, the water flies in an inſtant with vivacity, leaving the bottom of the veſſel uncovered; when it has reached the maximum of diſperſion it returns, and covers again the bottom of the veſſel which it had quitted.

Diſk of diſperſion.

2. I call that part of the veſſel which is abandoned by the water the *diſk of diſperſion.* In the preceding experiment, this diſk ſhows a perfect dryneſs and all its natural poliſh.

The repitition of the experiment leſſens its effects.

3. The repulſion becomes leſs conſiderable, and the diſk of diſperſion ſmaller, in proportion as the experiment is repeated in the ſame water. This ariſes from the water becoming gradually ſaturated with alcohol.

Influence of the veſſels.

4. The nature of the veſſels has no influence on the preceding experiment, nor on thoſe which follow. They take place equally in veſſels of porcelain, earthern ware, glaſs or metal.

5. The form of the veſſels has much influence on the ſecond period of the phenomenon, that is to ſay, on the return of the water, and on the diſappearance of the diſk of diſperſion.

If the veſſel is a little concave, the water always comes back, and covers the bottom of the veſſel again. It will be obvious, that this is the neceſſary effect of its gravity.

If the veſſel is flat, the water only returns when the diſk of diſperſion has not attained too great a diameter.

If the bottom of the veſſel is a little convex, the water does not return after having been diſperſed, and it muſt be evident, that to do ſo it would act contrary to the operation of its own gravity.

Motion of the expelled fluid;

6. Being deſirous to render the obſervation of the motions of the expelled fluid eaſier and more perfect, I ſubſtituted tincture of turnſole, which, as is known, is only water coloured by turnſole, for pure water. The reſults were the ſame, but much more ſenſible; and I could then readily diſtinguiſh an undulating or trembling motion on the internal edge of the water which ſurrounds the diſk of diſperſion; a motion which proves the continual emiſſion of the alcoholic particles againſt this interior edge, and determines the removal of the water.

and of the impelling fluid.

7. But to complete the proof of the explanation which I have juſt given of the phenomenon, it was alſo neceſſary to render the motions of the impelling fluid ſenſible: I ſucceeded

by an analogous proceſs. I coloured alcohol by means of turnſole; with this ſubſtance it takes a very beautiful colour which is not at all ſimilar to the violet blue of tincture of turnſole, but, on the contrary, is of a very vivid blue, analogous to that of indigo, or pruſſian blue. I then wetted the bottom of a plate with pure water, and, with a glaſs rod, brought a drop of this coloured alcohol to the centre: the water was driven back with vivacity. In the centre of the diſk of diſperſion was a blue ſpot, formed by the coloured alcohol, and the reſt of the diſk was white like the bottom of the plate. But the proof of the actual continual emiſſion of the coloured alcoholic particles, is that as the water retired, its internal edge, which touched the diſk of diſperſion, became more and more of a violet colour, analogous to that of tincture of turnſole prepared with water. It is evident, therefore that in natural philoſophy, facts are explained by facts, and, that this experiment confirms the conſequence I had deduced from the preceding experiment.

Water does not repel alcohol.

8. If the plate is wetted with the alcohol, and a drop of water is put into the centre, the alcohol does not experience any motion; the drop of water flattens, it retains its orbicular form for ſome moments, at length it finiſhes by ſpreading irregularly, mixing with the alcohol, and uniting with it.

Oil is repelled more ſlowly than water.

9. If the bottom of the plate is covered with a very thin ſtratum of olive oil, and a drop of alcohol is brought to the centre, the oil is repelled, though more ſlowly than the water on account of its viſcoſity, and the bottom of the plate is left dry.

If the ſtratum of oil is too thick, it will not quit the bottom of the plate, and the expanſive motion of the alcohol takes place only on the ſuperfices of the oil.

The eſſential oil of lemon or orange rind, produces repulſion, but weaker.

10. If a morſel of the freſh rind of lemon or orange is placed in the centre of a wetted plate, the water is ſenſibly repelled, and the diſk of diſperſion is agreeably tinged with the priſmatic colours, which depends on the diſengagement of the eſſential oil. But this motion has not nearly ſo much intenſity as that produced by means of alcohol.

The affinity of ammonia for water prevented its repulſive action,

11. Convinced by the preceding experiments that every volatile fluid at the atmoſpheric preſſure, was capable of producing this repulſive movement, I employed liquid ammonia. I therefore wetted the plate in the uſual manner, and brought

a drop of volatile alkali to the centre. What was my ſurpriſe to obſerve, that the ſlighteſt motion was not manifeſted in the water; I however ſuſpected the cauſe, and believed that this apparent anomaly was owing to the ammonia, which, having a very great affinity with water, combined with it at the inſtant of their contact.

which was exercifed in olive oil.

12. I reſolved, therefore, to ſubſtitute a fluid to the water, which had leſs affinity with ammonia, and I choſe olive-oil, which I had at hand. I again covered the bottom of the plate with a thin layer of this oil, and brought a drop of the ammonia to the centre: the oil was inſtantaneouſly repelled, as it had been by the alcohol.

Ammonia does not repel alcohol:

13. If the plate be wetted with alcohol, and a drop of ammonia is put into the centre, the alcohol is not repelled, the drop of ammonia flattens, and the two liquors evaporate. I thought I perceived a ſlight tremulation on the edge of the drop of ammonia.

but alcohol repels ammonia.

14. If the plate be wetted with ammonia, and a drop of alcohol is brought to the centre, the ammonia is repulſed like pure water. It appears, therefore, from this experiment, and the preceding, that the horizontal or lateral force of expanſion of the alcohol is ſuperior to that of the ammonia.

Olive-oil and water have no repulſive action.

15. A drop of olive oil put into a wetted plate, did not produce any motion in the water. It was the ſame with water beat up with oil to the conſiſtence of an unguent. *M. Prevoſt*, in his memoirs, ſeems to announce reſults obtained with the fixed oils, which are contrary to my experiments, and particularly to theſe.

I ſhall not enlarge farther on experiments which ſome may, perhaps, think more curious than uſeful. But when the attractive powers of yellow amber, or of the loadſtone were firſt obſerved, neither their importance, nor the aſtoniſhing diſcoveries to which they have ſince led, were ſuſpected.

XIV.

Letter from Mr. CUTHBERTSON *reſpecting his Galvanic and Electrical Experiments.*

To Mr. NICHOLSON.

DEAR SIR,

Introduction.

IN conſequence of the note which you have been ſo obliging as to add to my letter, addreſſed to Dr. Pearſon, inſerted in your Journal for this month, I have to ſay, that *the troughs were uſed collaterally:* I ſhall now be happy to ſee your remarks. It appears, that I ought to have been more explicit, and therefore, I beg leave to offer the following additional obſervations *.

The two laſt mentioned experiments in the letter alluded to, were compared with common electrical diſcharges, with a view to prove what quantity of coated glaſs would be requiſite to ignite the ſame lengths of wire.

Experiment. Deflagration of wire by jars.

Two jars, each containing about 170 ſquare inches of coated ſurface, were ſet to the conductor of a 24 inch ſingle plate electrical machine, with my univerſal electrometer loaded with 31 grains, (ſee Quarto Journal, *Plate* XXII. Vol. II.)

Pile of large plates.

I find in my notes of improvements for the 6th of June, 1803, that I had made a pile of 16 pairs of plates of 10 inches diameter, and that eight of them laid upon each other in the uſual manner, with cloths wetted with diluted muriatic acid, burned one inch of wire of 1-195th part of an inch in diameter, and that 16 pairs burned four inches of the ſame wire. This experiment was repeated on the 8th of June, with the ſame reſult, with reſpect to metals, but gave ſtrong and loud ſparks from metal to metal, ſufficient to be heard at 300 yards diſtance, which reſult, I believe, has never been obtained from troughs, ſo as to be heard, indeed, at any diſtance. For the laſt experiment, the cloths were wetted in a ſtrong ſolution of muriate of ammonia.

very loud galvanic ſparks.

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* I ought certainly to have mentioned the arrangement of the troughs, and likewiſe I ought not to have ſaid ſo vaguely, that double quantities of galvanic fluid, only burn double lengths of wire, becauſe I am ſtrongly of opinion, that the reaſon why galvanic diſcharges from troughs do not act upon metals in the ſame ratio as common electric diſcharges do, proceeds from ſome defect in the arrangement, and alſo conſtruction.

Eight inches of the ſame ſort of wire were laid in the circuit, 57 revolutions of the plate cauſed the electrometer to diſcharge the jars which ignited the wire perfectly, as in the ninth experiment. Then ſix inches of the wire were laid in the circuit, and the above number of revolutions cauſed the diſcharge, the wire being deflagerated and fuſed into balls in the ſame manner as in the eighth experiment.

Deduction as to the quantities of electricity in a jar and a pile.

Hence I conclude, that 340 ſquare inches of coated glaſs properly conſtructed, will bear a charge equal to a galvanic battery of 1080 ſquare inches of ſurface.

Probability that Mr. Wilkinſon's wire was thinner than ſtated.

The reſult of the above experiments gives me reaſon to think, that there is a miſtake reſpecting the diameter of the wire ignited by Mr. Wilkinſon's batteries, as mentioned in your Journal, Vol. VII. p. 297, to which you refer, becauſe to ignite one half inch of ſteel wire of *one ſeventieth of an inch in diameter*, will require a power ſufficient to ignite 120 inches of wire $\frac{1}{150}$ part of an inch in diameter, by common electrical diſcharges, which is a power equal to two of my common electrical batteries, (ſee your Quarto Journal, Vol. II. p. 525.)

The greateſt power of 60 pairs of 6 inches ſquare plates that ever has been known, was that of igniting 16 inches of wire of $\frac{1}{150}$ part of an inch in diameter. Mr. Wilkinſon's trough of 100 pairs of plates of 4 inches ſquare is of much leſs ſurface, and as he ſays, it is a leſs favourable ſize, from which, and from the above experiments, I conclude, that ſuch a battery has not the power of igniting one half an inch of wire *of one ſeventieth of an inch in diameter*, unleſs galvanic diſcharges act upon metals in ſome manner different to common electrical diſcharges, but with which I am unacquainted; perhaps Mr. Wilkinſon will be kind enough to clear up this remark.

I am, with due reſpect,

Dear Sir, Your very humble Servant,

JOHN CUTHBERTSON.

Poland Street, Soho,
June 19, 1804.

XV.

Chemical Examination of the Ochroites, a Mineral not hitherto well known, containing a New Earth. By KLAPROTH [*].

History of the foſſil.

THE foſſil which forms the ſubject of the preſent analyſis, and to which I have given the name of ochroites, for reaſons to be ſtated hereafter, is found in the mine of Baſnætes, near Riddarhytta in Weſtmannland.

Firſt noticed by Cronſtedt; and examined by Scheele

The firſt account of this mineral we owe to Cronſtedt, who furniſhed a deſcription of it, together with that of another mineral, found at Biſpberg in Delecarlien [†]. Scheele conſidered both as ſpecies of iron ores, and gives to them the name of lapis ponderoſus, ponderous ſtone, (ſchwerſtein) or tungſten, which he deſcribes in his mineralogy as *terrum calciforme, terra quadem incognita intime mixtum* [‡]. He likewiſe examined this tungſten, and made us acquainted with its true nature. The mineral which he examined, was, however, the pearl-coloured tungſten of Biſpberg, and from this he concluded, that the examination of the tungſten of Riddarhytta was neceſſary, conſidering it a mineral of the ſame nature, he diſtinguiſhed it by the name of *reddiſh tungſten.*

and D'Elhuyar.

Soon afterwards D'Elhuyar analyſed both minerals, he verified the analyſis of the true tungſten, but proved that the conjecture of Scheele concerning the other mineral was founded in error; the reſults of his analyſis ſhowed that the foſſil known by the name of reddiſh tungſten, was compoſed of 54 parts of lime, 24 of iron, and 22 ſiliceous earth. From what follows, it will, however, become obvious, that this mineral contains neither lime nor tungſten, but a new earth hitherto unknown.

External Characters of the Ochroites.

External characters of Ochroites.

The colour of this mineral is between carmoiſin red, clove-brown, and reddiſh-brown. It is compact, breaks, ſplintering in irregular, not very ſharp or angular pieces. It is perfectly opaque. Its powder is reddiſh-gray. It is not very hard, but brittle, and very ponderous.

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[*] Gehlen's new Journal of Chemiſtry, Vol. II. part. iii. p. 303.

[†] Tranſactions of the Swediſh Academy of Sciences, 1751. p. 235.

[‡] And alſo in Cronſtedt's Mineralogy by Magellen, Vol. I. p. 46.

Its ſpecific gravity is 4,660. Cronſtedt ſtates it to be =4,988.

A.

Analyſis of the Ochroites.

a. A piece of the mineral after having been ignited to redneſs, loſt 2 per cent. Its reddiſh colour had been changed to brown. Its figure had ſuffered no alteration.

b. One hundred grains of the finely levigated mineral ignited for half an hour, loſt five grains. Its colour was changed to a dark brown.

B.

a. One hundred grains of ochroit, after being mixt with 200 grains of carbonate of potaſh, were ſtrongly ignited, the maſs which could not be rendered fluid, was reddiſh, grey and brittle. On being diffuſed through water, as uſual, the obtained ſolution was colourleſs. It remained perfectly tranſparent; a proof that it did not contain tungſten oxide; nitrate of ſilver, mercury, lead, barytes, &c. proved the abſence of acids.

b. The inſoluble reſidue of the laſt proceſs was boiled in nitro-muriatic acid, the ſiliceous earth being ſeparated, the ſolution was decompoſed by potaſh, and the whole boiled for ſome time. The alkaline fluid after being neutralized with muriatic acid, and then mingled with carbonate of potaſh, ſuffered no charge.

C.

a. 200 grains of the finely pulverized mineral, were firſt boiled in two ounces of muriatic acid, to which half an ounce of nitric acid was gradually added, and the digeſtion continued for ſome time. The whole became thus diſſolved except the ſilex contained in the mineral. Its quantity amounted to 68 grains.

b. To the ſolution obtained in the laſt proceſs, carbonate of ammonia was added ſo long, till no permanent precipitate was produced. On letting fall into it ſuccinate of ammonia, a curdly precipitate fell, which vaniſhed again on agitation, leaving merely a pale red precipitate of ſuccinate of iron. This being collected, waſhed, dried, and ſtrongly ignited, yielded nine grains of oxide of iron.

c. The fluid thus freed from iron, and now colourleſs, was decompoſed by carbonate of ammonia. The precipitate obtained was white, and weighed 168 grains, on being deprived of water and carbonic acid by heat, its white colour changed to cinnamon-brown. It weighed 109 grains.

d. All the water employed for waſhing the different precipitates were mingled, evaporated to dryneſs, and the ammoniacal ſalt volatilized; a minute quantity of a muriate was obtained, the baſis of which could not be determined.

Peculiar earth.

From what follows it will become evident, that the cinnamon-brown precipitate (c.) which forms the principal part of the foſſil is a peculiar earth, diſtinct from all the others hitherto known. The characteriſtic property which it poſſeſſes of acquiring a light-brown colour after being heated, has induced me to call it ochroit earth*, which may alſo ſerve for the mineral itſelf.

According to this analyſis, 100 parts of the ochroite of Ridderhytta contain,

Ochroit earth - - -	54,50
Silex - - - - - -	34
Oxide of iron - - - -	4
Water, &c. (A. b.) - - -	5
Loſs - - - - - -	2
	100.

Characteriſtic Properties of Ochroit Earth.

Ochroit earth, 1. combines with carbonic acid.

1. Ochroit earth is capable of combining with carbonic acid during its precipitation from acids by carbonated alcalies, and ſtrongly conſolidating a portion of water.

100 grains of the earth precipitated by carbonate of ammonia, and ſtrongly dried, loſt on being neutralized by nitric acid, 23 grains: 100 grains of the ſame earth loſt after being ſtrongly ignited, 35 grains, 100 parts of carbonate ochroite therefore conſiſts of

Ochroit earth - - - - .	65
Carbonic acid - - - -	23
Water - - - - - -	12
	100.

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* From the Greek word ωχρος, (flaveſcens,) browniſh yellow.

2. It is brown.

2. Ochroit earth, after being freed from carbonic acid and water, by heat, always appears in the form of a cinnamon-brown powder. The intenſity of the colour is in proportion to the heat applied. This colour is not owing to the preſence of iron, or manganeſe, &c. but it is a characteriſtic property of the earth.

3. Not reducible.

3. Ochroit earth included in a charcoal crucible, and expoſed to the heat of the porcelain furnace, ſuffered no change whatever.

4. Not fuſible as the microcoſmic ſalt nor borax.

4. Urged by the blow-pipe, it becomes phoſphoreſcent; fuſed with phoſphate of ſoda and ammonia, it becomes tinged by it, without effecting a ſolution of the earth. The ſalt acquires merely a marbled lemon yellow colour. Borax has likewiſe no chemical effect upon it. This ſalt only effects a mechanical diviſion. The earth always appears diffuſed through the borax in minute *flocculi*.

5. Gives an uneven brown as a porcelain colour.

5. Ochroit earth mixed in different proportions with proper fluxes, and applied for painting of porcelain, proved unſucceſsful. The painted articles were light brown, but the colour was not uniform; a proof that no combination had been effected.

6. Difficultly ſoluble in acid if the earth be pure; but eaſily if carbonated.

6. Ochroit earth combined with carbonic acid is eaſily ſoluble with effervescence in acids. The taſte of the ſolution is very rough and aſtringent. The concentrated ſolution is of an amethyſt red colour; diluted with water, it becomes colourleſs. Ignited ochroit earth, on the contrary, is difficultly ſoluble in acids in the cold; if nitric acid be employed, the ſolution is yellowiſh red.

Nitric acid.

7. Sulphate of ochroit is cryſtallizable, and pale amethyſt colour.

7. The combination of ochroit earth with ſulphuric acid, is cryſtallizable. The figure of the cryſtals formed in the maſs of the fluids is the octahedron. They are heavy, of a pale amethyſt colour, and difficultly ſoluble in water; but the ſulphate of ochroit with exceſs of acid, is more ſoluble; the figure of the cryſtals formed on the ſides of the veſſel, are needle-ſhaped, radiating from a centre. They are more ſoluble than the former.

Sulphate of ſoda decompoſes mur. or nitrate of ochroite.

The inſoluble ſulphate of ochroite.

8. If a ſolution of ſulphate of ſoda be mingled with a ſolution of muriate or nitrate of ochroit, a mutual decompoſition takes place. A white inſoluble precipitate is formed, conſiſting of ſulphuric acid united to the ochroit earth. This com-

bination may be decompoſed by boiling it with double its weight of carbonate of ſoda. By this means ochroit earth may be obtained very pure.

may be decompoſed by boiling with carb. of ſoda; *and the earth obtained pure.*

9. Ochroit earth is likewiſe ſoluble in ſulphureous acid, the ſolution cryſtallizes in needles of a pale amethyſt colour.

9. Sulphureous acid diſſolves ochroit earth; cryſtallizable.

10. Muriatic acid diſſolves ochroit earth, and yields cryſtals, the figure of which is the priſm. It is ſoluble in alcohol without imparting to its flame any particular colour.

Muriatic acid alſo diſſolves it, and cryſtallizes. The alcohol ſolution does not colour flame.

12. Acetite of ochroit could not be cryſtallized, but yielded an adheſive maſs.

Acetite of ochroit not cryſtallizable.

13. Nitrate and muriate of ochroit is decompoſable by carbonated earths and alcalies, the precipitate is milk-white. Alcalies and earths freed from carbonic acid, occaſion a yellowiſh grey precipitate.

14. Pruſſiate of potaſh precipitates ochroit from all its neutral ſolutions, milk white. The precipitate is ſoluble in muriatic and nitric acid.*

The nitrate and muriate precipitable by earths and alkalis, and by pruſſiate of potaſh; white.

15. Tincture of galls occaſions no change in the ſolutions of this earth.

Not by galls,

16. Hidrogurettjed-hidroſulphuret of ammonia precipitates the ſolution of ochroit earth, yellowiſh white.

but by hidrog. hidro. ſulph. of ammonia.

17. Water impregnated with ſulphuretted hidrogen occaſions no change in the ſolutions of ochroit earth.

Not by hidro. ſulph. water,

18. Succinates precipitate ochroit earth white.

but by ſuccinates.

19. Phoſphate of ſoda, occaſions in the ſolutions of this earth a white precipitate, which again vaniſhes by the addition of nitric or muriatic acid.

Phoſphate of ſoda precipitates, &c.

20. Tartrites of potaſh alſo precipitate this earth white.

21. Oxalates effect a like decompoſition, the oxalate of ochroit, however, is not ſoluble in nitric or muriatic acids.

Tartrites precipitate it, and oxalates.

22. Alcalies and alcaline carbonates do not act on ochroit earth.

Alcalies do not act on the earth.

23. Ammonia feebly acts on it, under certain circumſtances, as may be evinced from the following experiment:

Ammonia feebly.

A ſolution of nitrate of ochroit, prepared by diſſolving 100 grains of carbonate of ochroit (not abſolutely free from iron) in nitric acid, was decompoſed by carbonate of ammonia, and digeſted in the fluid, containing a conſiderable quantity of carbonate of ammonia in exceſs, for ſome days. The fluid which had acquired a yellow colour, was ſeparated and neutralized by ſulphuric acid, and then placed in a warm place. A grey precipitate was thus obtained, which on being dried, weighed 1¼ grains. This precipitate, after being diſſolved in nitric acid, yielded a blue precipitate by pruſſiate of potaſh, this being ſeparated, a white flocculent precipitate fell down by dropping into the remaining fluid carbonate of potaſh. This method is therefore applicable for ſeparating a minute quantity of iron, that may be contained in the fluid.

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* If the earth contained the muriates and quality of iron, it becomes by this means manifeſted.

General remarks and characters of the ochroit earth.

From what has been ſtated, it becomes obvious, that the ochroit earth bears the neareſt relation to yttria, for like this it forms a connecting link between the earths and the metallic oxides. Like yttria it has the property of forming a reddiſh coloured ſalt with ſulphuric acid, and is precipitable by pruſſiate of potaſh, but it differs from yttria, that it does not form ſweet ſalts, that it is not (at leaſt very ſparingly) ſoluble in carbonate of ammonia, and that when ignited it acquired a cinnamon brown colour. It farther differs from yttria by not being ſoluble in borax or phoſphate of ſoda when urged upon charcoal before the blow-pipe, which ſalts eaſily effect a ſolution of yttria, and melt with it alſo into a pellucid pearl.

XVI.

Letter from VAN MARUM *to J. C. Delamétherie, on Ritter's Galvanic Experiments.*[*]

New experiment in proof of the identity of electricity and galvaniſm.

MR. ORSTED of Copenhagen, on his way through Harlem, having ſhewn me, by means of the apparatus in Teyler's Muſeum, ſome diſcoveries of his friend Ritter of Jena;[†] I made at the ſame time a new experiment with him, which affords a freſh proof of the identity of the fluid ſet in motion by Volta's pile and the common electrical machine, and an account of which I imagine will be acceptable for your Journal.

Two wires of platina, kept 5 minutes in the chain of communication between the two poles of the pile, convulſed the legs of a frog,

Mr. Orſted having ſhown me, that two wires of platina, after having been kept five minutes in the chain of communication between the two extremities of a galvanic pile, acquired thereby the faculty of throwing the legs and thighs of a frog into convulſive movements, according to the diſcovery of Mr. Ritter; I propoſed to him, to paſs the ſtream of fluid from a powerful electrical machine through theſe ſame wires of platina, in order to obſerve, whether it would have the ſame effect on them as the ſtream from the galvanic pile. We employed the plate machine of 35 inches diameter, and of the new conſtruction, deſcribed in the *Journal de Phyſique* for June, 1791, vol. 38, (or Philoſophical Journal, quarto ſeries, No. 2.)* Having paſſed the ſtream from it through the wires of platina, held a quarter of an inch from the conductor, for the ſpace of five minutes, we touched with them the crural nerves of a frog prepared in the uſual manner, and obſerved immediately the ſame convulſive movements in its legs and thighs, though not ſo ſtrong as in the preceding experiment. This leſs powerful effect of the ſtream from the machine perfectly anſwered my expectations, ſince my experiments in November, 1801, deſcribed in my letter to Mr. Volta, ſhewed me that the ſtream from the machine had not more than five ſixths of the velocity or ſtrength of the ſtream ſet in motion by a ſimilar pile.

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[*] *Journ. de Phyſique*, December, 1803, Vol. LVII. p. 471.

[†] See Ritter's experiments, as communicated by Dr. Orſted, in this preſent and ſome other numbers of the Journal.

and expoſed to the ſtream of a plate machine for a like time did the ſame, but more weakly.

We then repeated the experiment, keeping the wires of platina in contact with the conductor of the machine, while we paſſed the ſtream through them. Then holding the wires one in each hand, as in the preceding experiment, and in contact with the two crural nerves, but keeping their oppoſite extremities ſeparate, their effect on the ſame frog, the ſenſibility of which was greatly weakened, was ſcarcely perceptible: but on bringing the upper extremities of the wires together, while the lower remained in contact with the crural nerves, we noticed very ſtriking convulſions in the legs of the frog, every time the extremities were made to touch. A little ſealing wax, which had been uſed to keep the wires of platina inſulated when held to the conductor, and which ſtill adhered to one of the upper ends, rendered the experiment ſtill more evident; for when we brought theſe ends together in ſuch a manner, that a little of the wax prevented them from being in perfect contact, the legs of the frogs exhibited none of the

The experiment was repeated, the wires having been kept in contact with the conductor; when the ſenſibility of the frog being much weakened, the effect was ſcarcely perceptible, unleſs the upper extremities of the wires were brought into contact. When a little ſealing wax prevented the abſolute contact of the wires, no effect took place.

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* It is a machine, that produces poſitive and negative electricity, like the pile of Volta. *Delamétherie.*

convulſive movements, that took place, notwithſtanding every time the wires were made to touch each other completely.

Thus we have a freſh proof of the identity of the fluid ſet in motion by the pile of Volta, and by a common electrical machine.

XVII.

Experiments on Light; by Mr. Ritter, *of Jena. Communicated by Dr.* Orsted.[†]

Herſchel's diſcovery of inviſible ſolar rays has given riſe to another.

THE important diſcovery of inviſible ſolar rays, with which Herſchel has enriched natural philoſophy, has given occaſion to another little known, even in the country where it has been made.

Our knowledge of light at a ſtand ſince Newton;

till Herſchel found, that inviſible rays, beyond the ſpectrum, raiſed the thermometer. Ritter, repeating the experiment, conjectured, that light contained inviſible rays acting chemically likewiſe; and found, that muriate of ſilver, expoſed to the ſpectrum, grew blackeſt beyond its violet edge, and the effect diminiſhed in proportion to the diſtance from this, and the muriate a little diſoxigenated had its colour partly reſtored by the in-

Our knowledge of light had made no perceptible advancement ſince Newton, when Herſchel found, that all the phenomena occurring during the decompoſition of light by means of the priſm had not yet been noticed. Philoſophers had contented themſelves with perceiving different colours, without examining by other proceſſes, whether phenomena imperceptible to the eyes did not take place. Herſchel, by means of the thermometer, diſcovered inviſible rays exterior to the ſolar ſpectrum, that poſſeſs the property of raiſing the mercury. Mr. Ritter repeated his experiments with ſucceſs; but conſidering, that the different rays of light produce very different chemical changes in bodies ſuſceptible of them, he conceived, that light likewiſe contained inviſible rays, which acted chemically. He expoſed muriate of ſilver to the action of the ſolar ſpectrum, and found his conjecture ſubſtantiated: the muriate of ſilver very ſoon became black beyond the violet edge of the ſpectrum; blackened a little leſs in the violet itſelf; and this action was ſtill leſs in the blue, diminiſhing thus more and more in proportion to the diſtance from the violet, till it became null. On expoſing muriate of ſilver a little blackened, that is to ſay, a little diſoxigenated, to the ſame action of light, its white colour was partly reſtored by the red ray, and ſtill more by the inviſible ray beyond it.

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[*] *Journal de Phyſique*, December, 1803, Vol. LVII. p. 409.

The ſolar ſpectrum therefore is accompanied by two inviſible rays, one on the red ſide, which favours oxigenation, and the other on the violet ſide, which favours diſoxigenation. The viſible and coloured rays partake more or leſs of the properties of theſe inviſible principles, whence we ought to infer, that all the coloured rays contain more or leſs of theſe principles.

Theſe experiments ſucceed very well with phoſphorus. On letting the inviſible ray adjoining the red fall on it, it immediately emits white fumes: but if the inviſible ray adjoining the violet be thrown on the phoſphorus in a ſtate of oxigenation, it is inſtantly extinguiſhed, with the ſame rapidity as a frog is convulſed in galvanic experiments.

Theſe experiments readily accord with ſome others made by the ſame gentleman. He kept one of his eyes for ſome minutes in contact with the negative conductor of Volta's pile, and after this operation all objects appeared red to him: but after having kept it in contact with the poſitive conductor, he ſaw every thing blue. It is to be obſerved here, that the retina and optic nerve, when the external part of the eye is brought into the negative ſtate, become poſitive, and *vice verſa*; becauſe the eye is filled with a fluid, in which the ſame diſtribution of electricity muſt take place, as in water and other fluids. It is in the poſitive ſtate therefore, that the optic nerve perceives all objects of a red colour; and in the negative ſtate they appear violet. The chemical action of poſitive electricity likewiſe is the ſame as that of red light; that is, they both favour oxigenation. Negative electricity and the violet ray poſſeſs the ſame analogy, both promoting diſoxigenation: as the experiments with the galvanic pile have ſufficiently ſhown.

If I might be allowed to add any obſervation to theſe important diſcoveries, I would mention one of the moſt commonly known facts; that the oppoſite electricities, when united, produce light; which ſeems to demonſtrate ſynthetically, what the preceding experiments have ſhewn by analyſis.

This account was read to the philomathic ſociety ſeveral months ago, ſince which time Mr. Ritter has publiſhed ſome new obſervations that deſerve notice. He has found, with all the priſms he uſed, that the ſolar rays give two coloured ſpectrums, which ſpread in proportion to their diſtance from the priſm, ſo that at a certain diſtance one nearly covers the other.

viſible ray beyond the red. Thus there is an inviſible ray promoting oxigenation, and another the contrary, and the viſible and coloured rays partake more or leſs of the properties of both.

The inviſible ray on the red ſide likewiſe kindles phoſphorus; while that on the violet ſide extinguiſhes it inſtantly.

One of the eyes being kept ſome minutes in contact with the negative conductor of Volta's pile, all objects appeared red to it; in contact with the poſitive blue.

In the former caſe the optic nerve is in the poſitive ſtate; in the latter, negative.

The chemical action of poſitive electricity likewiſe favours oxigenation, like red light; and negative, the contrary.

The two electricities too produce light by their union; which ſeems to prove ſynthetically their identity with the chemical rays.

Mr. R. has ſince found, that the priſm affords two ſpectrums,

The

diſtinct at a little diſtance, as 4 inches, but confounded together when farther off.

The experiment muſt be made at a very ſmall diſtance from the priſm, that of four inches for example, to diſtinguiſh the two ſpectrums, which become confounded together in proportion to their diſtance from the priſm. No doubt this is the reaſon why the phenomenon has ſo long eſcaped the notice of philoſophers. This obſervation was accompanied by another, ſtill more important; namely, that the chemical rays may be completely ſeparated from the coloured rays. If the inviſible rays of the violet ſide be made to fall on the red part of the ſolar ſpectrum, the proceſs of oxidation may be completely ſuſpended there, and even diſoxidation produced, without deſtroying the red colour; and by means of ſeveral priſms we may even ſeparate all the coloured rays from the chemical rays. We may thus produce a coloured ſpectrum devoid of chemical action; and a ſeries of chemical rays, analogous to the ſpectrum, without any mixture of coloured rays.

And the chemical rays may be completely ſeparated from the coloured, without deſtroying them.

So that we may produce a coloured ſpectrum devoid of chemical action.

The calorific rays have not been proved to be ſeparable from the chemical; but it may be preſumed they are, ſince the former differ in degree in winter and ſummer, the latter do not.

We have not yet any accurate experiments, to determine whether the calorific rays be likewiſe ſeparable from the others, particularly from the chemical rays: but the compariſon of different experiments made in winter and ſummer, when the degrees of heat are different, though no difference in the force of the chemical rays at different ſeaſons has been obſerved, lead to a belief that the calorific rays are ſeparable from the chemical.

That they accompany each other, though ſeparable, no greater difficulty than attends electricity.

It may be aſked, why the different rays found in light frequently accompany each other, though they can ſubſiſt ſeparately: and this queſtion no doubt may be anſwered, when we are able to ſay, why the different functions of electricity accompany each other, though they likewiſe are ſeparable.

XVIII.

On Spontaneous Inflammations. By G. C. Bartholdi, Profeſſor of Phyſic and Chemiſtry.[*]

Definition of ſpontaneous inflammation.

SPONTANEOUS inflammation is that which is manifeſted in a combuſtible body, without the immediate contact of any ignited matter.

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[*] From the Annales de Chimie, Vol. XLVIII. p. 249, or No. 144.

Theſe kinds of combuſtion may be occaſioned by different cauſes, the principal of which are: Cauſes.

1. Conſiderable friction.

2. The action of the ſun.

3. The diſengagement of caloric, produced in bodies, which, though not combuſtible, by being brought near to other combuſtible bodies, may communicate ſuch a degree of heat to them, that they inflame by the contact of the air.

4. The fermentation of animal and vegetable ſubſtances heaped up in a large maſs, which are neither entirely dry, nor too wet, ſuch as hay, dung, &c.

5. The accumulation of wool, cotton, and other animal and vegetable ſubſtances, covered with oily matter, particularly drying oil.

6. The preparation of linſeed oil for printers' ink, of varniſhes, and in general of all fat.

7. The torrefaction of different vegetable ſubſtances.

8. The ſulphurated and phoſphorated hidrogen gaſes which are diſengaged in many of the operations of nature, and of which, the latter generally inflames by the ſole contact of atmoſpheric air, even at a low temperature, and which is often ſeen at the ſurface of the earth like a ſmall flame, known by the name of Jack o'Lantern, in places in which there are animal ſubſtances in a ſtate of putrefaction: if other combuſtibles are met with where the diſengagement takes place, they readily catch fire.

9. The phoſphuret of lime and potaſh, which may be formed in the preparation of charcoal, particularly in that from turf, and from ſome ſorts of wood which grow in marſhy ſituations. This charcoal by being wetted, or by ſimply attracting the humidity of the air, forms phoſphureted hidrogen gas, which, by the contact of the atmoſpheric air inflames, and may ſet fire to the whole maſs of charcoal.

10. The phoſphorus which is ſometimes, though rarely, formed in the carboniſation of different ſorts of wood, without combining either with lime or with potaſh in the ſtate of phoſphuret. Theſe charcoals do not inflame ſpontaneouſly at the common temperature of the atmoſphere, but they produce a detonation by percuſſion with nitrate of potaſh, or with ſome other nitrates and metallic oxides to which the oxigen adheres but feebly, and which, being in the ſtate of thermoxide, retain much latent caloric.

1. *Friction.*

Friction.

It is generally known that by rubbing two bodies againſt each other, they are heated; the intenſity of the produced heat depends on ſeveral circumſtances, and chiefly varies in proportion to the hardneſs of the friction, to the nature, and to the ſurface of the rubbing bodies: if the friction takes place between combuſtible bodies, ſuch as the woods, the heat which it excites is frequently ſufficient to inflame them: if the bodies are not combuſtible, ſuch as ſtones, or but little combuſtible, like the metals, they do not themſelves inflame, but they can communicate ſuch a degree of heat to other combuſtibles which ſurround them, that theſe can inflame by the contact of atmoſpheric air.

Dr. Palcani having repeated the experiments which have been long known for obtaining fire by the friction of two pieces of wood, to one of which he gave the form of a tablet, and to the other that of a ſpindle or a cylinder, he has allowed me to give the reſults of ſome of his experiments here, to ſhew that more attention ſhould be paid to the choice of the woods which are deſtined to rub againſt each other, in the conſtruction of machines and inſtruments.

Experiments with a tablet between two cylinders,

Two cylinders of		Tablet.		Duration.	Effect.
Box-wood	rubbed againſt	Box-wood	during	5 Minutes	Senſible heat
Idem		Poplar		*Idem*	*Idem*
Idem		Oak		*Idem*	*Idem*
Idem		Mulberry		3 Minutes	Cons. heat and ſmoke
Idem		Laurel		*Idem*	*Idem*
Laurel		Poplar		2 Minutes	*Idem*
Idem		Ivy		*Idem*	*Idem*
Ivy		Box-wood		3 Minutes	*Idem*
Idem		Hazle		*Idem*	*Idem*
Olive		Olive		*Idem*	*Idem*
Mulberry		Laurel		2 Minutes	Con. he. ſm. & black
Aſh		Oak		5 Minutes	Senſible heat
Idem		Fir		*Idem*	*Idem*
Pear-tree		Oak		*Idem*	*Idem*
Cherry-tree		Elm		*Idem*	*Idem*
Plum-tree		Apple-tree		*Idem*	*Idem*
Oak		Fir		*Idem*	*Idem*

and with a cylinder between two tablets.

On changing the experiment, and rubbing a cylinder of one of theſe woods between two tablets of the other, for example, a cylinder of poplar between two tablets of mulberry, the augmentation of the rubbed ſurfaces which are in contact with the air, produced a much more conſiderable heat, and nearly all the woods mentioned above took fire.

The direction of the grain of the wood affects the friction.

The effect of the friction alſo varies according as the woods employed, particularly if they are of the ſame ſpecies, are rubbed in the direction of the grain of the wood, or when the grains of the woods croſs each other. In the firſt caſe the friction and the heat are much more conſiderable than in the ſecond.

Preventives againſt ſpontaneous inflammation by friction.

In large machines, in which there is a great deal of friction, the heating is prevented by directing a continual current of cold water on the rubbing ſurfaces: in common machines, and in coaches, waggons, *&c.* it is diminiſhed by covering the rubbing ſurfaces with ſome oily matter. There have been many examples, during the great heat of ſummer, of coaches, and other machines ſubjected to a rapid motion, having taken fire, becauſe the greaſing them had been neglected. The greaſe, by hardening on the rubbing ſurfaces, inſtead of diminiſhing the friction, increaſes it; and as this covering is very combuſtible, it renders ſpontaneous inflammation ſtill more eaſy. It is alſo preferable, in many circumſtances, to rub machines with ſoap, talc, plumbago, or other ſubſtances, which, without being oily, are very unctuous to the touch.

2. *Action of the Sun.*

Action of the ſun.

By concentrating the ſolar rays with convex glaſſes or concave mirrors, the ſtrongeſt heat is produced, all ſorts of combuſtibles are ſet on fire, and the moſt refractory ſubſtances are melted: it may happen that other bodies may be found in circumſtances, in which, without our concurrence, they produce the effects of glaſſes and burning mirrors: although theſe effects are rather phyſical than chemical, it is nevertheleſs eſſential to make them known, to guard againſt their danger. There are examples of fires produced by large glaſs bottles, filled with water and expoſed to the ſun, in an apartment. Whenever the form of the veſſel is nearly ſimilar to that of a lenticular or ſpherical glaſs, the rays are refracted, and by uniting in a focus, produce a heat capable of ſetting fire to the combuſtible bodies within it.

3. *The Heat excited in Bodies not combuſtible.*

Heat from incombuſtible bodies.

It is known that quick-lime plunged into water, or ſimply moiſtened, produces a conſiderable heat. This method has even been employed with ſucceſs for heating apartments, green-houſes, hot-beds, *&c.* at little expence. This property of quick-lime of diſengaging much heat by the contact of water, and that, not leſs dangerous, of diſſolving or corroding animal ſubſtances immerſed in it, require the greateſt precautions when a conſiderable quantity of quick-lime is kept together. To preſerve it, it muſt be protected from the contact of the air and of every ſpecies of humidity, and it muſt be carefully kept at a diſtance from all combuſtible bodies, ſuch as wood, hay, ſtraw, *&c.* which might inflame ſpontaneouſly, if the lime contracted the leaſt humidity. The *Journal de la Haute-Saône* gave an account, laſt year, of the deſtruction of a barn, one of the wooden partitions of which took fire from a heap of quick-lime, intended for the repairs of the farm, having been careleſsly laid againſt it.

In nature a great number of analogous phenomena occur, in which bodies, by changing their compoſition, or by contracting new combinations, heat ſo much, or diſengage ſuch a quantity of caloric, that other combuſtibles which are near them may take fire.

(To be continued.)

XIX.

Diſcovery of two new Metals in crude Platina. By SMITHSON TENNANT, *Eſq. F. R. S.*

New metal *Iridium.*

AT the laſt meeting of the Royal Society a paper of Mr. Tennant was read, on the analyſis of the black powder which remains after diſſolving platina, ſhewing that it contains two new metals. Mr. Tennant's firſt experiments were made laſt ſummer,

ſummer, and had been communicated to Sir Joſeph Banks, after which an account of one of theſe metals appeared in France, by M. Deſcotil*, and alſo by M. Vauquelin. The properties aſcribed to it by the French chemiſts are, 1. That it reddens the precipitates of platina made by ſal ammoniac; 2. That it diſſolves in marine acid; 3. That it is precipitated by galls and pruſſiate of potaſh. The properties mentioned by Mr. Tennant are, that it diſſolves in all the acids, but leaſt in marine acid, with which it forms octahedral cryſtals. The ſolution with much oxigen is deep red, with a ſmaller proportion green or deep blue. It is partially precipitated by the three alkalies when pure. All the metals, excepting gold and platina, precipitate it. Galls and precipitate of potaſh take away the colour of this ſolution, but without any precipitate, and afford an eaſy teſt of its preſence. The oxide therefore loſes its oxigen, by water alone. When combined with *gold or ſilver, it cannot be ſeparated by the uſual proceſs of refining theſe metals.* As the French chemiſts have not given a name to the metal, Mr. Tennant inclines to call it *Iridium,* from the various colours of it in ſolution.

New metal *Oſmium.*

The ſecond new metal is obtained by heating the black powder with pure alkali in a ſilver crucible. The oxide of this metal unites with the alkali, and may be expelled by an acid and obtained by diſtillation, being very volatile. The oxide has a very ſtrong ſmell, from which Mr. Tennant has called it *Oſmium.* It does not redden vegetable blues, but ſtains the ſkin of a deep red or black. The oxide in ſolution with water has no colour, but by combining with akalli or lime becomes yellow. With galls it gives a very vivid blue colour. All the metals, excepting gold and platina, precipitate this metal. If mercury is agitated with the aqueous ſolution of the oxide, an amalgum is formed, which, by heat, loſes the mercury, and leaves the oſmium pure as a black powder.

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* See our Journal, Vol. VIII. p. 118.

SCIENTIFIC

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

Figure of the Orbits of the new Planets. By JEROME DE LALANDE.

Figure of the orbits of the new planets.

THE mean diſtance of each of theſe planets from the ſun is 2,77, which anſwers to 227 million of geographical miles nearly.

Piazzi or Ceres, diſcovered Jan. 1, 1801.

Revolution 4 years, 7 months, 10 days.

Mean longitude, Jan. 1, 1804 -	$10^{s.}$	11°	59′
Annual motion - -	2	18	14
Aphelion - - -	10	26	44
Node - - -	2	21	6
Equation of the orbit - -		9	3
Eccentricity - - -		0′,	079
Inclination - - -		10	37

Albers or Pallas, diſcovered March 28, 1802.

Revolution 4 years, 7 months, 11 days.

Mean Longitude, Jan. 1, 1804 -	$9^{s.}$	29°	53′
Annual motion - -	2	16	11
Aphelion - - -	10	1	7
Node - - -	5	22	28
Equation of the orbit - -		28	25
Eccentricity - - -		0,2463	
Inclination - - -		34	39

Philoſophical Tranſactions of the Royal Society of London, for the Year 1804. *Part I.* 4to, 182 *Pages, with five Plates and* 26 *Pages of Metereological Journal.*

Philoſophical Tranſactions of the Royal Society.

THIS Part contains—1. The Bakerian Lecture, Experiments and Calculations relative to Phyſical Optics; by Thomas Young, M. D. F. R. S. 2. Continuation of an Account of a peculiar Arrangement in the Arteries diſtributed on the Muſcles of flow moving Animals, &c.; in a Letter from Mr. Anthony Carliſle to John Symmons, Eſq. F. R. S. 3. An

Philoſophical Tranſactions of the Royal Society.

3. An Account of a curious Phenomenon obſerved on the Glaciers of Chamouny; together with ſome occaſional Obſervations concerning the Propagation of Heat in Fluids; by Benjamin Count Rumford, V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. 4. Deſcription of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall; with ſome Obſervations upon the various Modes of Attraction which influence the Formation of Mineral Subſtances, and upon the different Kinds of Sulphuret of Copper; by the Count de Bournon, F. R. S. and L. S. 5. Analyſis of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall, by Charles Hatchett, Eſq. F. R. S. 6. Obſervations on the Orifices found in certain poiſonous Snakes, ſituated between the Noſtril and the Eye; by Patrick Ruſſell, M. D. F. R. S.: with ſome Remarks on the Structure of thoſe Orifices, and the Deſcription of a Bag connected with the Eye met with in the ſame Snakes; by Everard Home, Eſq. F. R. S. 7. An Enquiry concerning the Nature of Heat, and the Mode of its Communication; by Benjamin Count Rumford, V. P. F. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. 8. Experiments and Obſervations on the Motion of the Sap in Trees; in a Letter from Thomas Andrew Knight, Eſq. to the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S. Appendix, Metereological Journal kept at the Apartments of the Royal Society, by Order of the Preſident and Council.

Analytical Eſſays towards promoting the Chemical Knowledge of Mineral Subſtances. By MARTIN HENRY KLAPROTH, *Profeſſor of Chemiſtry, Aſſeſſor to the Royal College of Phyſicians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Vol. II. 8vo, 267 Pages. Tranſlated from the German.* Cadell and Davies.

Analytical Eſſays.

THIS work is tranſlated by the ſame learned chemiſt to whom the ſcientific world is obliged for the former volume. Its valuable contents are as follow. 73. Examination of the Auriferous Ores from Tranſylvania. 74. Analyſis of the ſulphated Oxyd of Manganeſe from Tranſylvania. 75. Examination of Tungſtate of Lime (Scheelium). 76. Gadolinite. 77. Examination of the Egyptian Natrum (Soda). 78. Stri-

ated

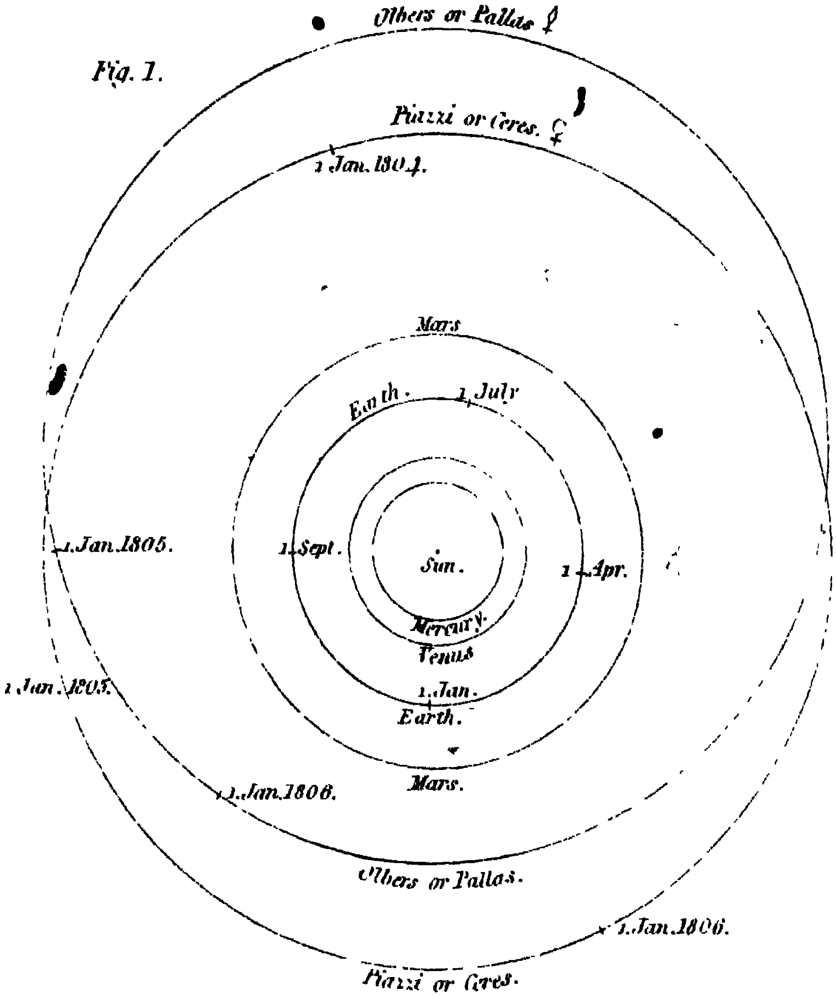
Analytical Eſſays.

ated Soda. 79. Analyſis of the native Muriate of Ammoniac. 80. Examination of Saffolin. 81. Examination of the Plumoſe Alum from Freyenwalde. 82. Capillary Salt (Halotrichium) from Idria. 83. Elaſtic Bitumen, from Derbyſhire. 84. Examination of Mellilito. 85. Umbra (Umber). 86. Examination of the muriated Lead Ore. 87. Phoſphated Lead Ores. 88. Sulphated Lead Ores. 89. Tabular, White Lead Ore, from Leadhills. 90. Examination of the native Reguline Antimony, from Andreaſberg. 91. Antimoniated Silver, from Andreaſberg.* 92. Fibrous *red* Antimonial Ore. 93. *White* Ore of Antimony. 94. *Arſeniated* Olive Copper Ore. 95. *Muriated* Copper Ore. 96. *Phoſphated* Copper Ore. 97. *Kryolite.* 98. Beryl. 99. Emerald. 100. Examination of Klingſtone (Echodolite). 101. Baſalt (Figurate Trapp). 102. Pitch Stone. 103. Addition to the Analyſis of Pumice Stone (Eſſay 33). 104. Examination of the Jargon (Zircon) from Norway. 105. Examination of Madreporite. 106. Pharmacolite. 107. Scorza. 108. Examination of the Fibruous Sulphate of Barytes. 109. Tabular Spar (Safel-ſpath). 110. Examination of Miemite. 111. Examination of the *priſmatic, Magneſian Spar*, from the Territory of *Gotha*. 112. Examination of the ſtriated grey Ore of Manganeſe. 113. Earthy, black Oxyd of Manganeſe. 114. Examination of the Aſphaltum from Albania. 115. Earthy brown Coal. 116. The Hungarian Pearl Stone.

⁂ W. N. acquaints G. S. that the ſuppoſition of Du Hamel, that what was called mild volatile alkali contains chalk, aroſe from a want of knowledge of the carbonic acid at the early period alluded to in his letter. The additional weight in the carbonate of ammonia ariſes from carbonic acid, and there is no reaſon to ſuſpect any volatilization of the earth during its preparation. If any were preſent, it might be detected by ſulphuric acid, or, in preference, by oxalic acid, either of which would carry down the lime.

Sketch of the Orbits of the new Planets, by Jerome de la Lande.

Fig. 1.



Arseniated Copper

Fig. 3.

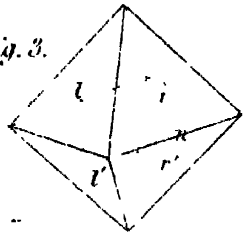


Fig. 2.

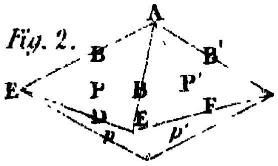


Fig. 4.

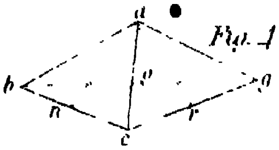
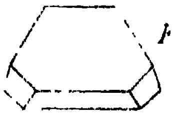
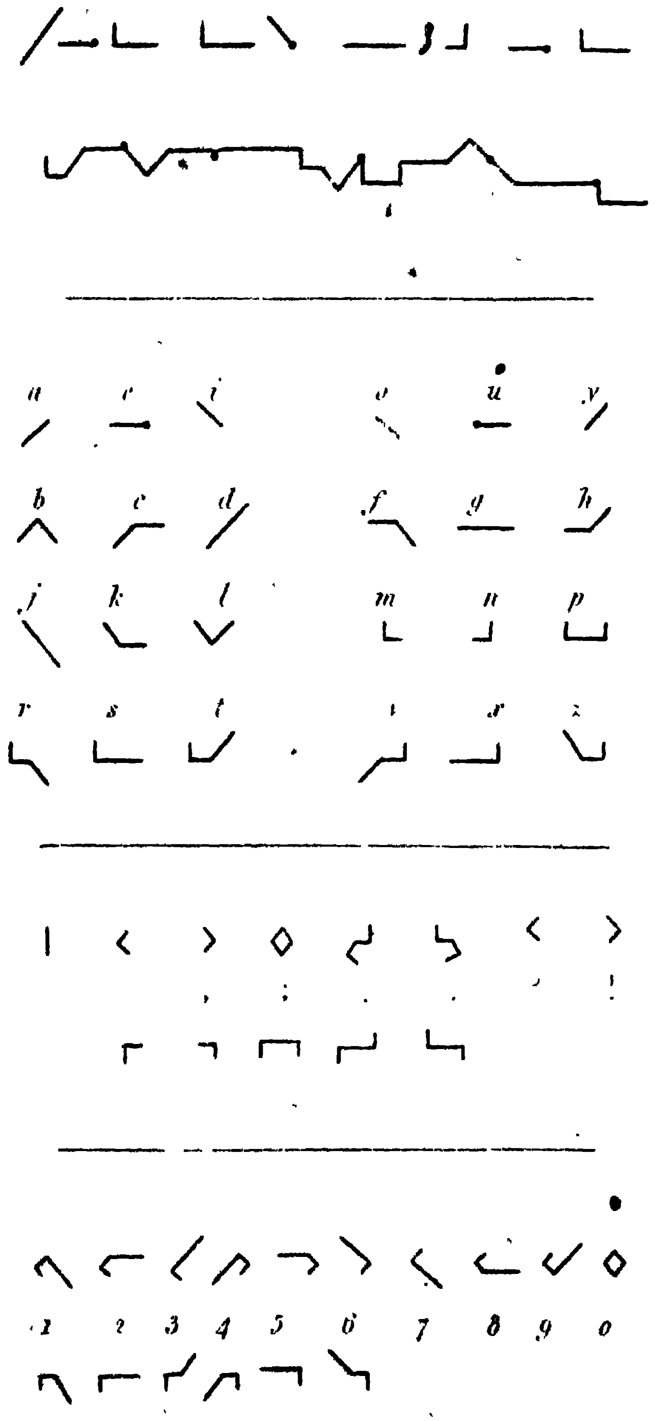


Fig. 5.



Telegraph by the human figure.



Mutlow sc. Russell Ct.

Machine for laying Land level,
by Mr David Charles.

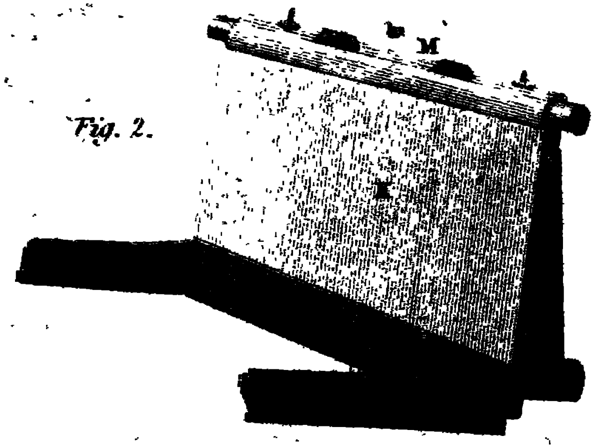


Fig. 2.

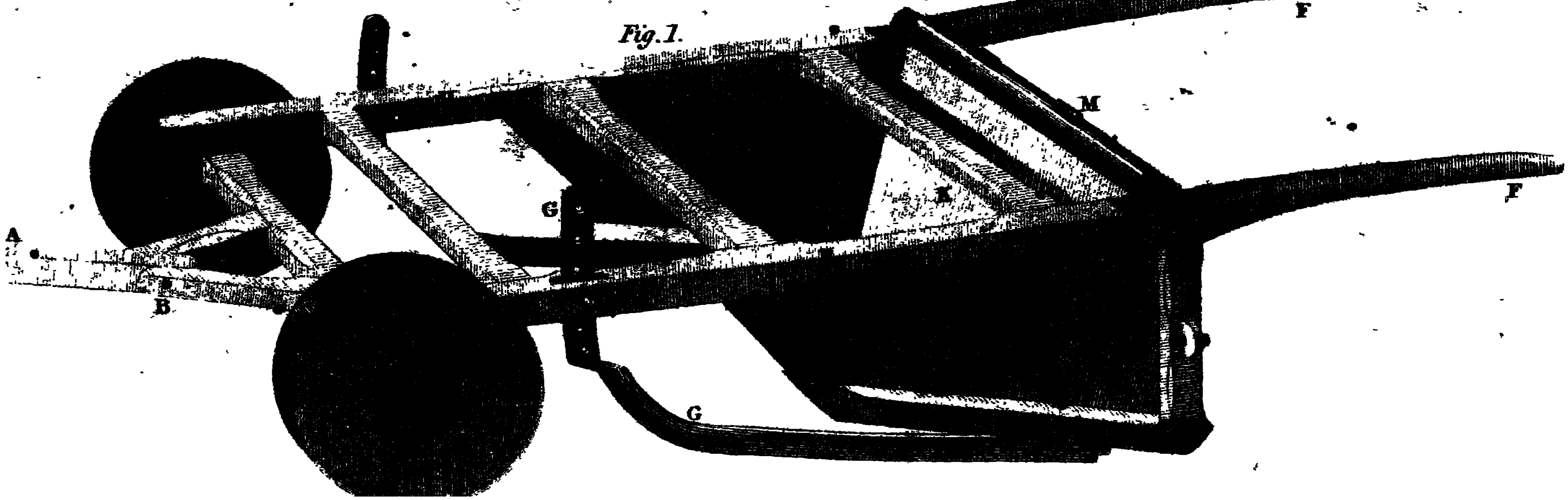
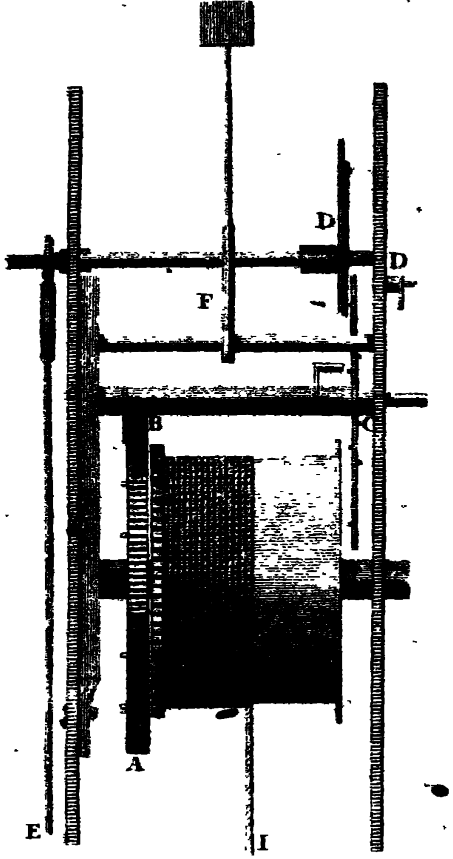


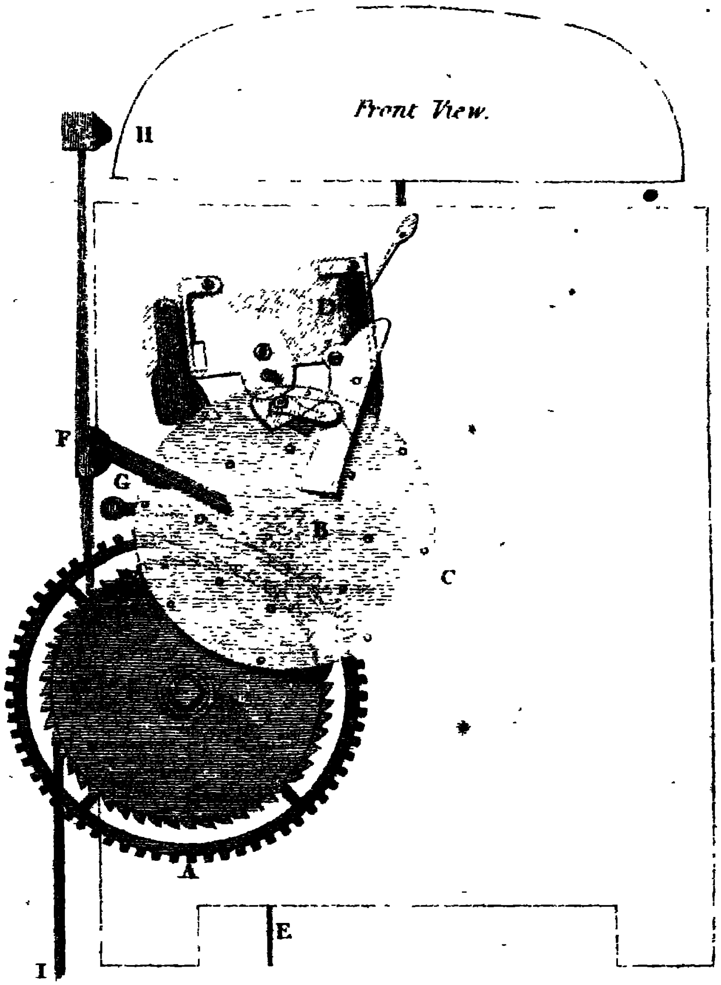
Fig. 1.

Clock to strike without a Fly, by Mr. E. Massey.

Side View.



Front View.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1804.

ARTICLE I.

On the Paſtes, coloured Glaſſes, or Enamels, of the Ancients.

By M. KLAPROTH.*

SECTION FIRST.

Hiſtory of the invention of glaſs.

Pliny's Account.

THE invention of glaſs, which, in various reſpects is ſo highly valuable a production of art, is among thoſe ancient diſcoveries of which hiſtory has tranſmitted us ſome account. Pliny relates it as follows: † "*Fama eſt, adpulſa nave mercatorum nitri, cum ſparſi per littus epulas pararent, nec eſſet cortinis attollendis lapidum occaſio, glebas nitri e nave ſubdidiſſe. Quibus accenſis, permixta arena litoris, tranſlucentes novi liquoris fluxiſſe rivos; et hanc fuiſſe originem vitri.*" If we ſuppoſe this account to be merely an unſupported tradition, ſtill it contains in itſelf no circumſtance that might render it queſtionable. It deſerves rather the more credit, as it is hardly poſſible to imagine this invention could have had any other origin than that of accident.

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* Read in the Royal Academy of Sciences at Berlin, October 4, 1798.—Tranſlated by N. N. who received a copy from the author.

† Lib. XXXVI. Cap. 65.

Glaſs-houſes at Dioſpolis in the moſt remote ages,

and among the Phenicians.

Though Pauw and ſome other antiquaries are more inclined to aſcribe this diſcovery to the Egyptians, who are ſaid to have conſtructed the firſt glaſs-houſe, in the remoteſt ages, at Dioſpolis, the ancient capital of Thebaïs; yet it likewiſe appears, from the writings of the ancients, that this art muſt have arrived at a conſiderable degree of perfection, chiefly among the Phenicians; as alſo, in general, this nation ſeems, in her flouriſhing age, to have been almoſt in the excluſive poſſeſſion of manufactures. Sidon, that colony of theirs ſo flouriſhing by commerce, arts, and manufactures, was not leſs famous on account of her glaſs-houſes. Theſe, according to the teſtimony of Pliny, obtained for ſome hundred years the chief ingredients of their glaſs from the ſea-ſhore near the Phenician town Acco, afterwards called Ptolemais, and now St. John d'Acre, in the vicinity of the ſmall river Belus, which there empties itſelf into the Mediterranean.

The nitrum or natrum of the ancients was ſoda;

or rather it conſiſted of any ſalt left upon the ground (not muriatic);

This impure ſalt was not detrimental to the glaſs.

The ſubſtance which the ancients employed for the purpoſe of vitrifying the ſand, is comprehended by the early authors under the name of *nitrum*; but it has long been generally agreed, that they did not mean by it our nitrate of potaſh, but the mineral alkali or ſoda; conſequently their *nitraria* were not nitre-works, but, ſtrictly ſpeaking, refineries of ſoda. And from the deſcriptions which Pliny and others have given of their *natrum* and its properties, it is rendered probable, that in thoſe times all ſaline ſubſtances, whether effloreſcing upon the ſoil or left by dried lakes, if not belonging to the muriatic genus, were conſidered as natrum. Hence undoubtedly, among thoſe ſalts often occurred real nitre as well as native ſulphate of ſoda. However, ſuch confuſion in their uſe in manufacturing glaſs has not produced any real detriment; ſince the longer time during which the ancients expoſed their materials for glaſs to the action of the fire, has been more than ſufficient to decompoſe thoſe neutral ſalts, and to expel from them their acid conſtituent parts.

The art of colouring glaſs is of nearly the ſame antiquity as glaſs itſelf.

The art of colouring glaſs ſeems to be of nearly the ſame antiquity as the invention of making it; as is evident not only from ſeveral paſſages in the ancient writers, but may alſo be proved by actual documents, and, among others, by the variouſly coloured glaſs-corals, with which ſeveral of the preſerved Egyptian mummies are decorated. This art ſuppoſes the poſſeſſion of ſome chemical knowledge of the metallic oxides, becauſe theſe are the only ſubſtances capable of producing ſuch an effect. But it would be a problem of difficult ſolution to determine, what were the means and proceſſes employed by the ancients for this purpoſe, as they had no acquaintance with the mineral acids, which, at preſent, are uſually employed in the preparation of metallic oxides. It is nevertheleſs certain, that the art of giving many various colours to glaſs muſt, at leaſt in later times among the Greeks and Romans, have reached an eminent degree of perfection; for they knew how to imitate, by their paſtes of glaſs, even thoſe gems which have a deep colour, ſo as to deceive the eye very conſiderably. A proof of this, among others, is afforded by the following words of Pliny*, relating to the artificial imitation of the carbuncle, a gem then in the higheſt eſtimation: "*Adulterantur vitro ſimillime: ſed cote deprehenduntur, ſicut aliæ gemmæ factitiæ.*"

Though the ancients were ignorant of our mineral acids, they made coloured paſtes in high perfection.

It was in the time of Auguſtus that the Roman architects began to make uſe of coloured glaſs in their moſaic decorations, beſides the ſeveral ſpecies of marble and other coloured ſtones, which, before, were uſually employed with that deſign. Such an application of the glaſs-paſtes was reſorted to in a villa built by the emperor Tiberius on the iſland of Capri, as is ſhewn by ſpecimens lately found among its ruins. I ſubjected ſome of theſe in my poſſeſſion to chemical analyſis, chiefly for the purpoſe of diſcovering what metallic ſubſtances the ancients employed to tinge thoſe variouſly coloured maſſes of glaſs.

Roman moſaic work of glaſs.

I. *Antique Red Glaſs.*

The colour of this glaſs-paſte is a lively copper-red. The maſs is perfectly opake, and very bright at the place of recent fracture. This is probably the very ſame glaſs, of which Pliny ſays:* "*Fit et totum rubens vitrum, atque non tranſlucens, Hæmatinon adpellatum.*"

Antique red glaſs; opake.

(a) Two hundred grains of this red glaſs were finely triturated, and, together with 400 grains of *cauſtic potaſh*, ignited for half an hour; by which management the mixture ſoon entered into a thin fuſion. After cooling, the whole maſs was ſoftened with water, then ſuperſaturated with *muriatic* acid; and, after this mixture had been again inſpiſſated to a ſaline

Analyſis, 200 grains, triturated and fuſed with 400 gr. potaſh; ſoftened with water; ſuperſaturated with muriatic acid; inſpiſſated; diffuſed in much hot water.

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* Lib. XXXVII. Cap. 26.

maſs, it was again diffuſed in a large quantity of boiling water, to which a ſlight portion of muriatic acid had previouſly been added. *Siliceous earth* ſeparated; which, collected, edulcorated and ignited, weighed 142 grains.

Silex fell down.

The remaining ſolution depoſited cryſtals of muriate of *lead* by evaporation.

(*b*) The filtered ſolution poſſeſſed a green colour; and when concentrated by evaporation, it depoſited needle-ſhaped cryſtals. When on continuing the evaporation no more ſuch cryſtals would appear, the remaining fluid was diluted with ſpirit of wine, and thrown upon the filter. The collected cryſtals were waſhed with ſpirit of wine and dried in a warm temperature, upon which they weighed 32½ grains. They conſiſted of muriated lead, equivalent to 28 grains of gently ignited *oxide of lead.*

The liquid was then ſuperſaturated with ammonia, and *alumine* was ſeparated; The rem. fluid being ſat. with mur. acid, then evap. and precip. by oxalate of potaſh, gave ox. of *lime.*

(*c*) I then ſuperſaturated with *cauſtic ammonia* the ſolution thus freed from its contents of lead. It was now of a dark-blue colour, and let fall a grey precipitate; which being ſeparated, the ſolution was again neutralized with muriatic acid, reduced by evaporation, and upon this combined with *oxalate of potaſh* as long as any turbidneſs enſued. The precipitate thence formed was oxalate of lime, which after ſtrong ignition yielded three grains of *pure calcareous earth.*

Copper was then precip. by *iron.*

(*d*) The ingredient copper was now precipitated from the ſolution, by immerſing into it a poliſhed piece of iron. The reguline copper obtained by this proceſs amounted to twelve grains, for which fifteen grains of oxided copper muſt be put in the account.

Purification of the precipitated alumine.

(*e*) The above grey precipitate (*c*) thrown down by the cauſtic ammoniac, was mixed and digeſted with liquid *cauſtic ſoda.* When to the filtered ſolution, again ſuperſaturated with muriatic acid, *carbonate of ſoda* was added, *aluminous earth* fell down, which after waſhing and ignition amounted to five grains.

Inſoluble part was *iron.*

(*f*) The remaining part that was left undiſſolved by the cauſtic lye, appeared of a black-brown tinge. This, after waſhing and expoſure to red heat, weighed two grains, and was *oxided iron.*

Hence, according to this analyſis, the ſum of the conſtituent parts of the *two hundred* grains of the *red antique glaſs-paſte* decompoſed, conſiſts of,

Silex

Compoſition of the red glaſs.

Silex	(a)	142	grains.
Oxide of Lead	(b)	28	——
—— *of Copper*	(d)	15	——
—— *of Iron*	(f)	2	——
Alumine	(e)	5	——
Lime	(c)	3	——
		195	

It was probably a ſcoria from copper works.

On comparing the external characters of this red glaſs-paſte with the cupreous ſcoriæ of a lively brown-red, ſuch as is ſometimes obtained on melting copper ores, it is rendered highly probable, that the ancients did not compound the above paſte directly from its ſimple conſtituent parts, but inſtead of them have perhaps employed copper ſcoriæ. On that ſuppoſition they had nothing more to do, than to ſelect the beſt coloured pieces to fuſe and caſt them into plates.

II. *Antique Green Glaſs-Paſte.*

Antique green glaſs; opake. Analyſis as in §. 1.

The colour of this green paſte is a light verdigris. Its maſs, like that of the preceding, is opake, and of a ſcoriaceous ſplendent fracture.

For its chemical analyſis I employed *two hundred* grains, which, having been treated in exactly the ſame manner as the foregoing, I found to conſiſt of the following ingredients:

Component parts of the green enamel;

Silex	130	grains.
Oxide of Copper	20	——
—— *Lead*	15	——
—— *Iron*	7	——
Lime	13	——
Alumine	11	——
	196	grains.

Differ only in proportion of parts from the green.

This green glaſs-paſte, then, contains the ſame conſtituent parts as the red, only in different proportions. Both receive their colour from copper: But the reaſon why this metal produces in the one a red, and in the other a green colour, depends on the different degrees of its oxidation or ſaturation with oxigen.

It is one of the chemical properties of copper, that in the ſtate of a ſub-oxide, that is only half ſaturated with oxigen, it produces a copper-red enamel; while, on the contrary, when perfectly oxided or fully ſaturated with that acidifying principle, the enamel which it yields is green. Pliny mentions ſeveral preparations of copper that were in uſe in his time; he only dwells too long on enumerating their pretended medicinal virtues. Of ſuch artificial preparations of copper ſome might have been ſerviceable in making green glaſs-paſtes, in the caſe that, perhaps, the native oxides of copper, of which in particular the copper-mines on the iſland of Cyprus could afford copious quantities, were not then employed for this purpoſe.

III. *Antique Blue Glaſs.*

Whether the ancients coloured glaſs by cobalt.

My leading object in chemically decompoſing this glaſs, was the ſolution of the queſtion: What was the colouring matter which the ancients employed in order to tinge their glaſs blue? The ſtriking ſimilarity of the colour of the blue antique glaſs to that of our modern, which, as is well known, is tinged by means of cobalt, has induced ſeveral learned men to conjecture, that even the ancients muſt have been acquainted with this foſſil, as well as with its properties of colouring glaſs blue. This was, likewiſe, the opinion of Ferber, when in his *Letters from Italy*, page 114, he ſays: "*In the villa Adriani near Tivoli, near Freſcati, and in ſeveral places, antique muſaic works have been found which exhibited ſome cubes of a blue vitreous compoſition, and ſerve as a proof that the ancients muſt have known the uſe of cobalt and the preparation of ſmalt.*" This opinion he repeats in various places.

The affirmative generally,

but erroneouſly ſuppoſed.

This opinion being ſupported by no chemical proof, reſts ſolely on the ſuppoſition, that cobalt is the only ſubſtance which is capable of affording a blue enamel. However, it is certain the ancients knew the art of giving, by means of iron, a blue colour to glaſs reſembling that which we produce by cobalt.

The contrary ſhewn by Gmelin,

A chemical demonſtration of this fact has been given by Gmelin of Göttingen, in his *Chemical Examination of a Blue Glaſs from an antique Moſaic Fragment* [*], which was found in digging a garden at Mümpelgard, and is probably of Roman origin. It is true that Gmelin could, in his examination, employ no more than the ſmall quantity of a few grains; but the reſults were ſufficient to ſhew, that the colouring principle in his ſpecimen originated not from cobalt but from iron.

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[*] Commentat. Götting. Vol. II.

and by our author.

A like reſult is afforded by the following decompoſition of the blue glaſs from the ruins at Capri.

Antique blue glaſs; almoſt perfectly opake.

Some plates are blue only to a certain depth.

Its colour is a *ſapphire-blue* verging towards that of ſmalt. It is tranſparent on the edges only. Its fracture, as well as that of the preceding, comes nearer to the ſcoriaceous and conchoidal than to the ſplintery. Some of theſe blue glaſs-plates are particularly diſtinguiſhed by this circumſtance, that they are not coloured blue throughout the whole of their maſs, but only to about two-thirds of their thickneſs. Each of the ſtrata is ſo nicely diſtinct from the other, as to give the appearance of two plates adhering at their broad ſurfaces; the one blue, the other colourleſs.

Fuſion with ſoda, &c. gave *ſilex*.

(a) *Two hundred* grains of the above blue paſte were reduced to a ſubtile powder, and fuſed with 400 grains of *cauſtic ſoda*. The obtained maſs, ſoftened with water, was ſaturated to exceſs, and evaporated to a moderate dryneſs. When rediſſolved in boiling water, it depoſited *ſiliceous earth*, which, after waſhing and ignition, amounted to 163 grains.

Alumine by the proceſs with ammonia, &c.

(b) The fluid was then ſuperſaturated with *cauſtic ammonia*. A brown precipitate thence enſued, which, upon edulcoration, I digeſted with a ſolution of *cauſtic potaſh*. The ſlight portion taken up of it by this alkali was again, after ſaturating the lixivium with an exceſs of muriatic acid, precipitated by means of *carbonated ſoda*, and proved, upon edulcoration and red heat, to be *aluminous earth*, amounting to three grains.

Oxided iron.

(c) What remained undiſſolved by the cauſtic potaſh, was merely *oxided iron*, weighing nineteen grains when ignited and waſhed.

The liquid contained a little copper and lime; but *no cobalt*.

(d) The liquor that had been ſuperſaturated with cauſtic ammonia and poſſeſſed a blueiſh tinge, was by ſlow evaporation ſo far reduced to a ſmaller volume, that the greateſt part of the muriate of ſoda, which had been generated and contained in it, could ſeparate in cryſtals. The fluid ſeparated from theſe, in which the acid predominated, and which now hardly exhibited any perceivable greeniſh colour, was in vain examined for cobalt *. It contained only a ſlight trace of copper and lime. The firſt of theſe was made to appear, by combining the fluid with *pruſſiate of potaſh*, and the brown-red precipitates thus obtained amounted to a little more than two grains, which are to be conſidered equivalent to about one grain of *oxided copper*.

(e) At laſt, carbonated ſoda threw down about half a grain of *calcareous earth*.

Component parts of the blue paſte as enamel.

In conſequence of this decompoſition, thoſe *two hundred* grains of the antique blue glaſs-paſte, muſt have contained the following earthy and metallic conſtituent parts:

Silex - -	*(a)*	-	163.	grains.
Oxide of Iron	*(c)*	-	19.	— —
Alumine - -	*(b)*	-	3.	——
Oxide of Copper	*(d)*	-	1.	——
Lime - -	*(e)*	-	0.5	——
			186.5	

Other experiments were made but ſhewed no cobalt.

Iron can afford a blue enamel; as is ſeen in the ſmelting works.

As I have ſubjected the above blue glaſs to ſeveral other experiments, merely with an intention of diſcovering the cobaltic portion it might poſſibly contain, yet without finding the leaſt trace of it, there appears to be no doubt, that its blue colour entirely depends on the ingredient iron. That iron, under ſome circumſtances, is capable of producing a blue enamel, is clearly ſhewn by the beautifully blue coloured ſcoriæ of iron, which frequently are met with in the high furnaces on ſmelting ſiliceous iron-ſtones. But we are not ſufficiently acquainted with the circumſtances and conditions under which this colour is produced; for the aſſertion of Henckel, and ſome other earlier authors, that by means of iron, cemented with arſenic, the ſame blue tinge can be given glaſs which it acquires from cobalt, has not yet been ſufficiently confirmed. Whence, after the diſcovery of the blue from cobalt, the art of tingeing glaſs blue by means of iron has had the ſame fate with ſeveral other attainments now loſt; namely, to have been diſcarded and forgotten on the account of new invented, more commodious, and certain expedients and methods.

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* It is well known that nature tinges the *ſapphire, lapis-lazuli, blue clays*, &c. by means of *iron* without cobalt; but man is not poſſeſſed of her means. A chemical friend, with whom the tranſlator had a converſation on this ſubject and on the difficulty of proving the accuracy of the above *analyſis* by a *ſynthetical proceſs*, ſuggeſted the idea, that ſuch a blue paſte could, perhaps, be made without cobalt by the intermedium of lapis-lazuli; an idea which may afford a ſubject for experiment.—*Tranſl.*

SECOND SECTION.

The preceding glaſſes reſemble our enamels;

THESE coloured muſaic glaſs-paſtes of the ancients agree, with reſpect to their opacity and ſcoriaceous fracture, with our modern enamels. On the other hand, the deceitful imitations of gems already mentioned before ſhew, that the ancients likewiſe knew how to prepare beautiful, high-coloured, and tranſparent glaſs-paſtes.

but the ancients poſſeſſed another method of painting, little known to antiquaries:

It is formed of delicate fibres of glaſs united by fuſion:

But however well known thoſe works in glaſs of the ancients may be, ſince both earlier and later writers have given ſufficient information of them, and ſeveral ſpecimens preſerved in the collections of antiquaries afford a direct knowledge of this ſubject; it is, on the contrary, very ſurpriſing, that antiquaries are ſo little acquainted with that entirely peculiar and by far more remarkable painting on glaſs, which is formed of variouſly coloured delicate glaſs fibres, joined with the greateſt nicety, and by ſubſequent fuſion conglutinated into an homogeneous compact maſs. In the earlier works on antiquities this ſcarce production of art is not at all mentioned; the reaſon of which is probably this, that the ſpecimens now exiſting of it were found, perhaps, only about the middle of this *(laſt)* century.

Firſt mentioned by Count Caylus.

Ample deſcription by Winkelmann.

Among later antiquaries Count Caylus appears to be the firſt, who in his *Collections of Antiquities* has given information, accompanied by rather inadequate drawings, of this ſingular ſpecies of moſaic work. Winkelmann has afterwards, in his *Annotations on the Hiſtory of the Art among the Ancients*, (page 5, *ſeq.*), more accurately deſcribed two other antiques of this kind, with the appellation, *Pictures made of Glaſs-Tubes*, in the following paſſage: "The works of the ancients in glaſs, which are not noticed in the *Hiſtory of the Arts*, deſerve particularly to be mentioned in this place; more eſpecially, becauſe the ancients carried the art of working in glaſs to a much higher degree than we have arrived at; a fact which, to thoſe who have not ſeen their works of this kind, might have the appearance of a groundleſs aſſertion." After which he mentions a floor formed of green glaſs-plates diſcovered in the Farneſe-iſland, as well as ſome fragments of glaſs-cups, which muſt have been turned on the lathe, and then proceeds as follows: "But the art ſtrongly claims our admiration in two ſmall pieces of glaſs, which laſt year (1765) were brought to Rome. Each of them is not quite one inch long, and one-third of an inch broad. One plate exhibits, on a dark ground of variegated colours, a bird repreſenting a duck of various very lively colours, more ſuitable to the Chineſe arbitrary taſte, than adapted to ſhew the true tints of nature. The outlines are well decided and ſharp, the colours beautiful and pure, and have a very ſtriking and brilliant effect; becauſe the artiſt, according to the nature of the parts, has in ſome employed an opake, and in others a tranſparent glaſs. The moſt delicate pencil of the miniature painter could not have traced more accurately and diſtinctly, either the circle of the pupil of the eye, or the apparently ſcaly feathers on the breaſt and wings, behind the beginning of which this piece had been broken. But the admiration of the beholder is at the higheſt pitch, when, by turning the glaſs, he ſees the ſame bird on the reverſe, without perceiving any difference in the ſmalleſt points; whence we could not but conclude, that this picture is continued through the whole thickneſs of the ſpecimen; and that, if the glaſs were cut tranſverſely, the ſame picture of the duck would be found repeated in the ſeveral ſlabs; a concluſion which was ſtill farther confirmed by the tranſparent places of ſome beautiful colours upon the eye and breaſt that were obſerved. The painting has on both ſides a granular appearance, and ſeems to have been formed, in the manner of muſaic works, of ſingle pieces; but ſo accurately united, that a powerful magnifying-glaſs was unable to diſcover any junctures. This circumſtance, and the continuation of the picture throughout the whole ſubſtance, rendered it extremely difficult to form any direct notion of the proceſs or manner of performing ſuch a work. And the conception of it might have long continued enigmatical, were it not that, on the ſection of the fracture mentioned, lines are obſervable, of the ſame colours which appear on the upper ſurface, that pervade the whole maſs from one ſide to the other; whence it became a rational concluſion, that this kind

Very curious antique enamel of a duck;

traced and finiſhed with extreme accuracy and effect;

and continued through the whole thickneſs of the piece, minutely the ſame on both ſurfaces!

It is found to conſiſt of threads of glaſs ſeen endwiſe.

of painting muſt have been executed by joining variouſly coloured filaments of glaſs, and ſubſequently fuſing of the ſame into one coherent body. The other ſpecimen is of about the ſame ſize, and made in the ſame manner. It exhibits ornamental drawings of green, white, and yellow colours, which are traced on a blue ground, and repreſent volutes, beads, and flowers, reſting on pyramidally converging lines. All theſe are very diſtinct and ſeparate, but ſo extremely ſmall that even a keen eye finds it difficult to purſue the ſubtle endings, thoſe in particular in which the volutes terminate. Notwithſtanding which, theſe ornaments paſs uninterruptedly through the whole thickneſs of the piece."

Another ſpecimen no leſs admirable.

The ſame were ſeen by Sulzer.

Of the ſame glaſs-paſte, which has been here deſcribed, mention is made by Sulzer in his *Theory of the Polite Arts*, under the article *Muſaic* (moſaïch). Having ſeen the piece itſelf in the houſe of its then poſſeſſor, Caſanova, at Dreſden, he confirms, in the capacity of an eye-witneſs, the deſcription given by Winkelmann, and calls it " a remnant of antiquity, which indicates the exiſtence of an art brought to the higheſt degree of perfection."

Extremely minute bird in Mr. Townley's collection.

Mr. Townley, of London, enumerates, among the principal rarities of his celebrated cabinet of antiquities, the ſtone of a ring of a ſimilar antique glaſs-paſte, which repreſents a bird of ſo ſmall a delineation, that it cannot be diſtinctly ſeen but by means of a magnifying lens.

Two ſpecimens in poſſeſſion of the author.

As very few ſpecimens of this ſpecies of glaſs-painting, which undoubtedly muſt be reckoned among the loſt attainments of art, and of which even the exiſtence is ſtill ſo little known, are met with; I think it not ſuperfluous to give the following notice of two new ſamples which I poſſeſs of this claſs of antique ſubjects. Both pieces have a heart-ſhaped form. The principal front is flat, the reverſe is convex, and has from eight to ten prominences *(Ecken)*. The length of one of them is one inch, the breadth four-fifths, and the thickneſs two-fifths of an inch. The other ſpecimen is *two-thirds* ſmaller. As to colouring and manner of drawing, they are both nearly alike. The principal maſs of the *larger* is of a dark-blue, and wholly opake; but that of the *ſmaller* is a ſapphire-blue, and in ſome places tranſparent. The blue ground is embelliſhed with voluted, ſtellular, minute flowers, of ſo very ſmall a delineation as to be hardly imitable by the

Deſcription.

pencil of the miniature painter. The colours of theſe flower-like ornaments, which are red, green, brown, ſky-blue, and white, are pure and lively. The delineations pervade the whole ſubſtance, and upon a broken part it is ſeen by mere inſpection, that theſe delicate figures have been formed of parallel glaſſy fibres of various colours, conglutinated by means of gentle fuſion.

Coloured plate of the larger ſpecimen.

As the drawings given by Count Caylus afford but a very imperfect idea of that ingenious enamel-painting, I ſubmit here a delineation of the larger of my two ſpecimens. *Fig.* 1, *Plate* XIII. repreſents it in its true ſize; *Fig.* 2 exhibits it magnified.

Invention of Meyer to multiply copies of paintings.

On this occaſion the following paſſage from a letter of M. Käſtner *, concerning an invention of the celebrated Tobias Mayer, may well deſerve a place here. "Mayer poſſeſſed the art of making a number of perfectly ſimilar copies of a painting. He compounded his picture of coloured *wax-crayons*, in the ſame manner as a priſm may be compoſed of thinner priſms of the ſame length. Every tranſverſe cutting afforded then a copy." Meyer might probably have been led to this method of imitation in wax, from the inſpection of a moſaic work of the kind here deſcribed.

II.

On Spontaneous Inflammations. By G. C. BARTHOLDI, *Profeſſor of Phyſic and Chemiſtry.*

(Concluded from Page 220.)

4. *The Fermentation of Animal and Vegetable Subſtances.*

Heat from fermentation.

THE greater part of animal and vegetable ſubſtances, when they ſtill retain humidity, and are accumulated into large maſſes, enter into fermentation, a change in their compoſition is effected, and they frequently heat to the point of inflammation. It is thus that magazines of hay, of turf, of flax, of hemp, ſtacks of hay or ſtraw, heaps of linen-rags in paper-mills, &c. take fire ſpontaneouſly.

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* *Allgemeine Geographiſche Ephemeriden*, by Zach, 1798.

It is principally hay which requires precaution; if the hay-harveſt happen in rainy weather, it is commonly ſtacked before it is thoroughly dry, and in this ſtate is more diſpoſed to ferment and to heat. If a hay-ſtack is obſerved to be in fermentation, great care muſt be taken not to throw it down too ſuddenly, the exterior layers muſt be carefully detached one after the other. It almoſt always happens, that when a hole is made in the middle of a ſtack of heated hay, it takes fire ſuddenly.

Utility of adding ſalt to hay.

Nothing, however, is more eaſy than to prevent theſe fatal accidents: when there is any reaſon to fear that the hay which is intended to be houſed or ſtacked, is not ſufficiently dry, it is only neceſſary to ſcatter a few handfulls of common ſalt (muriate of ſoda) between each layer. It would be very ill judged to regret this trifling expence; for the ſalt, by abſorbing the humidity of the hay, not only prevents the fermentation and conſequent inflammation of it, but it alſo adds a taſte to this forage which ſtimulates the appetites of cattle, aſſiſts their digeſtion, and preſerves them from many diſeaſes.

During the great heats of ſummer, it frequently happens that heaps of dung inflame ſpontaneouſly: great care ſhould be taken to ſprinkle them frequently with water in the ſummer ſeaſon, and to keep them at a certain diſtance from habitations, as well to prevent fires as with a view to ſalubrity.

5. *The Accumulation of Animal and Vegetable Subſtances covered with an Oil.*

Heat from greaſy animal and vegetable matters.

If animal and vegetable ſubſtances heaped into a large maſs, can take fire from the heat produced by their decompoſition, this accident is ſtill more to be dreaded when they are covered with oily matters, and particularly with a drying oil.

Beſides the accident which happened at the manufactory of Lagelbart, and of which our colleague Hauſſman gave an account to the Society, and the fire which took place in one of the fineſt manufactories at St. Marie-aux-Mines, there are many other examples of wool, ſtuffs, and pieces of cloth, which were not freed from greaſe, taking fire in the warehouſes when they were folded together, and even while moving them from one place to another when they were in large quantities: this is principally to be dreaded when linſeed oil, or any other oil, drying in itſelf, or rendered ſo by oxide of lead, is employed in the preparation of theſe ſtuffs.

In the manufacture of cloths, only olive oil, or oil of colza, ſhould be uſed to greaſe the wool.

It ſometimes happens on boiling flowers and herbs in oil, which occurs in ſeveral pharmaceutical operations, that, after being taken out, the herbs dried in the oil inflame ſpontaneouſly: care ſhould therefore be taken when theſe herbs are thrown away, not to heap them near other combuſtible bodies.

There have been ſeveral examples of veſſels having been burnt in ſea-ports, either by the ſpontaneous combuſtion of heaps of cordage coated with tar, or by a mixture of linſeed oil boiled with lamp-black, and incloſed in a bag.

6. *The boiling of oily Matters.*

Fire from boiling oily matters;

In the preparation of ſome varniſhes, ſuch as printers' ink, in which linſeed-oil, boiled to a certain conſiſtence, is generally made uſe of, it frequently happens that the oil takes fire, unleſs the neceſſary precautions are employed: the ſame effect takes place in melting butter, lard, or any other greaſe, if they are heated too much; ſo that, in theſe operations, it is always neceſſary to remove every other combuſtible ſubſtance, to have a lid at hand to cover the veſſel as ſoon as the fire has caught, and particularly to take care not to pour water upon it, which inſtead of extinguiſhing it, would ſpread it more and give it greater activity.

7. *Torrefaction.*

and from roaſted vegetable ſubſtances.

There are many vegetable ſubſtances which increaſe their power of inflaming ſpontaneouſly by torrefaction, if they are incloſed in ſacks of cloth, which leave them in contact with the ſurrounding air; ſuch are ſaw-duſt, roaſted coffee, the meal of grain, and leguminous fruits, ſuch as French beans, lentils, peaſe, *&c.*

There have been ſeveral inſtances of ſtables having taken fire from a bag of roaſted bran which had been applied to the neck of a ſick animal, and had inflamed ſpontaneouſly. The inhabitants of the country who, in ſome diſorders of their beaſts, are obſtinate in applying this remedy, to which others of more efficacy and leſs danger might be ſubſtituted, ſhould at leaſt be careful not to incloſe the bran in the cloth, either too hot or too much roaſted.

Brewers, after having made their barley and other grain, which they employ in making beer, germinate, dry it in a kiln, except what is intended for pale beer, and they generally roaſt it more or leſs highly, to give the beer a deeper or paler colour. If, therefore, when the grain is brought from the kiln, it is put, ſtill hot, into ſacks, it frequently happens that it heats and takes fire, and this has occaſioned ſeveral fires in breweries.

8. *Sulphurated and phoſphorated Hidrogen Gas.*

Inflammation of ſulphurated and phoſphorated hidrogen gas.

The cauſe of ſubterraneous fires and volcanoes is generally attributed to the decompoſition of pyrites, or metallic ſulphurets, buried in the interior of the earth. Theſe pyritous maſſes are decompoſed by the contact and concurrence of water and air, and the decompoſition is always accompanied by a great expanſion of caloric, and a diſengagement of a very inflammable gas, called ſulphurated hidrogen gas. This gas inflames at an elevated temperature, and can communicate the inflammation to the ſulphur of the pyrites, to the coal and other bituminous matters which generally accompany it.

Similar inflammations are ſometimes obſerved near coal-pits. In exploring the coal, veins and inſulated maſſes of pyrites are frequently met with: ſince theſe pyrites always communicate a bad quality to the coal, the miners generally lay them aſide, and throw them out of the pit: if theſe heaps of pyrites, mixed with coal, are then expoſed to the alternate action of the ſun and rain, they heat and inflame. Great care muſt therefore be taken that theſe accumulations of pyrites ſhould be kept at a diſtance from all combuſtible bodies to which they would neceſſarily communicate the inflammation.

There are many operations of nature in which ſulphurated hidrogen gas is formed, but it often enters into other combinations as it forms, it diſſolves in water, or diſengages at a temperature too low for it to inflame.

By boiling phoſphorus in a ſolution of potaſh or lime, phoſphorated hidrogen gas is diſengaged, which, being much more combuſtible than ſulphurated hidrogen gas, inflames at a low temperature as ſoon as it comes in contact with atmoſpheric air.

This gas, which in chemical experiments offers the beautiful ſpectacle of a fountain of fire over water, is produced naturally by the putrefaction of animal ſubſtances which are buried. The lights which are frequently ſeen to come out of the earth, and which are known by the name of Jack o' Lanterns, are only owing to the diſengagement of this phoſphorated hidrogen gas: as theſe lights generally appear moving about in places where they do not touch dry combuſtibles, they ſeldom occaſion diſagreeable accidents; but they are alſo diſengaged in foreſts, and it may happen that in hot ſummers, when the graſs and bruſh-wood are thoroughly dry, the gas in combuſtion may meet with theſe combuſtible materials, and ſet fire to them, and thus produce the conflagration of a whole foreſt: we ſhould not therefore, too lightly, and without ſufficient proofs, attribute to the malevolence or to the connivance of mankind, thoſe dreadful events which are ſometimes only the reſult of cauſes purely natural.

9. *Sulphuret and Phoſphuret of Lime and of Potaſh formed in the Combuſtion of ſeveral Vegetables.*

Sulphuret and phoſphuret of lime or potaſh.

Sulphur is always formed when gypſum (ſulphate of lime) or any other ſulphate, whether earthy or alkaline, is ſtrongly heated with charcoal, wood, or, generally, with any combuſtible which is reduced to charcoal by heat. The ſame ſalts form ſulphureous waters if the remains of animal or vegetable ſubſtances are left in a water in which it is diſſolved; ſo that it frequently requires only a little ſulphate of lime, or ſome other ſulphuric ſalt, to communicate the odour and taſte of ſulphur to water which is ſtagnant.

Pyrophorus.

Pyrophorus is obtained by calcining the alum of commerce, or ſulphate of potaſh with ſugar, meal, or any other ſubſtance which is reducible to charcoal.

The inflammation of pyrophorus which takes fire by the ſole contact of humid air, is only owing to the ſulphuret of potaſh, which by attracting the humidity of the air, heats to that point that it kindles the carbonaceous matter which ſurrounds it, and which being in a ſtate of greater tenuity, is ſo much the more diſpoſed to burn.

A pyrophoric matter may be formed in ordinary combuſtion.

But ſince many of our common combuſtibles contain ſulphuric ſalts, it may happen that, in their combuſtion, a pyrophoric matter is ſometimes formed by chance, which remains in the reſidue of the combuſtion, eſpecially if the combuſtible is not entirely conſumed, and a part is not reduced into charcoal, which frequently happens in the fire-places in which the combuſtibles are not burned in grates, and the aſhes are not ſeparated from the charcoal. There have been inſtances of houſes having been burnt by aſhes intermingled with the charcoal which had been taken too early from the fire-place and put into places where they were ſurrounded with combuſtibles, which they ſet on fire by a ſpontaneous inflammation. Happily theſe cauſes of conflagration rarely occur; for pyrophorus does not retain its property of inflaming for any length of time, and it is frequently decompoſed ſhortly after its production, without occaſioning any unpleaſant event: nevertheleſs, care ſhould always be taken not to put aſhes newly burned, and which are ſtill intermingled with charcoal, in places where they can communicate with combuſtibles.

The formation of a pyrophoric matter is principally obſerved in the preparation of the ſoda of commerce, which is obtained by the incineration of different maritime plants containing much ſulphate of ſoda, and which, in the combuſtion, ſometimes furniſh a certain quantity of ſulphur, greater or leſs, according to the manner in which the operation is directed.

The formation of phoſphuret of lime has great analogy with that of ſulphuret of lime. Although the phoſphoric acid is not found ſo often in vegetables as the ſulphuric acid, it nevertheleſs exiſts in them in greater quantity than has hitherto been believed: it is principally found in moſt plants which grow in marſhy places, in turf, and in ſeveral ſpecies of the white woods. In reducing theſe woods into charcoal, a ſmall quantity of phoſphorus is ſometimes formed, which may remain combined with the ſame baſes as retained the phoſphoric acid before the combuſtion: the phoſphorus, by forming other combinations, may be rendered incapable of occaſioning any accident, but it may alſo happen from a concurrence of ſeveral circumſtances, that charcoal impregnated with any phoſphuret whatever, may, by expoſure to the action of a warm and humid air, diſengage phoſphorated hidrogen gas, which, by the contact of the atmoſpheric air, will take fire and communicate the inflammation to the maſs of the charcoal.

Phoſphoric acid is abundant in vegetables.

Two examples of this kind of ſpontaneous combuſtion have taken place in the powder magazine of Eſſone, in the years 8

Spontaneous inflammation of charcoal in a powder magazine.

and 10. The firſt time the receiver of the machine for ſifting the charcoal caught fire, and the ſecond time it began in the magazine of charcoal, without a ſuſpicion of any other cauſe except that of a ſpontaneous inflammation. The different reports made on theſe two events, have been inſerted in the public journals; but the explanations that were given of them were not ſatisfactory. It ſeems very probable that they were occaſioned by ſome phoſphorus contained in the charcoal; and this explanation has the more weight, becauſe willow (bourdaine), which is uſed at Eſſone, as well as in moſt other powder-manufactories, and which, in many reſpects, deſerves the preference over other woods in the preparation of powder, contains phoſphoric acid, at leaſt that does which grows in our neighbourhood.

Charcoal from turf begins to be employed in ſome domeſtic and other operations; but as it is much diſpoſed to ſpontaneous inflammation, its uſe ſhould be prevented, or at leaſt it ſhould be ſtored with great precaution. It has happened at Paris and other places, that magazines of this charcoal, which were unſheltered, have taken fire by the combined action of the heat and rain.

10. *Phoſphorus ſometimes contained in Charcoal.*

Detonation from the phoſphorus in charcoal.

It may alſo happen that the ſmall quantity of phoſphorus which is ſometimes formed in the carbonization of different ſorts of wood, without uniting either with the lime or the potaſh, remains combined with the charcoal, which then does not diſengage phoſphorated hidrogen gas, nor does it readily inflame by the ſole action of water or of a humid air, but which, by percuſſion with ſalt-petre (nitrate of potaſh) may produce a powerful detonation. It is very probable that the three ſucceſſive exploſions which took place in the powder-mill at the manufactory of Vonges, were partly owing to a ſimilar cauſe.

Charcoal probably one cauſe of the bad quality of cold-ſhort iron.

Charcoal has, in general, great influence on the different productions of nature and the arts. It is frequently obſerved in forges and founderies, particularly in thoſe of iron, that the products vary according to the nature of the charcoal employed. The bad quality which is ſometimes found in iron, of being brittle when cold, is generally attributed to the phoſphoric acid contained in the ores: but ſince the ſame ore, by the ſame proceſſes,

ceſſes, furniſhes better iron in one foundery than in another, the difference ſeems frequently to ariſe in part from the charcoal.

Such are the principal cauſes of ſpontaneous combuſtions, whoſe effects are ſo much the more dangerous by being leaſt expected. The Society of Emulation thought they ſhould render an important ſervice to every claſs of proprietors, and particularly to the inhabitants of the country, by developing the phyſical knowledge which might guard them againſt dangers, of which they are too frequently the victims, from ignorance and a fatal want of foreſight. I truſt I have fulfilled the wiſh of the Society, and of the firſt Magiſtrate, whoſe intelligence and conſtant ſolicitude extend, without exception, to every object which may contribute to the proſperity of the country and the happineſs of the governed.

III.

On the Solution of Water in the Atmoſphere; and on the Nature of atmoſpherical Air. By Mr. JOHN GOUGH. *From the Author.*

To Mr. NICHOLSON.

SIR,

I DO not recollect any philoſopher or meteorologiſt, who has attempted to demonſtrate the chemical union of atmoſpherical air and water, by help of the following facts and arguments. Should the preſent endeavour, to eſtabliſh the propoſition, appear deſerving of a place in your Journal, the inſertion of it will oblige,

Yours, &c.

JOHN GOUGH.

Middleſhaw, July 16, 1804.

Dry vegetables attract water.

Exp. 1. If a cylinder of dry porous wood be put into a ſtrong glaſs tube, nearly of the ſame diameter with itſelf, and water be poured into the veſſel, the particles of the fluid will penetrate the wood, and cauſe it to ſwell, ſo as to burſt the glaſs. Some writers affirm, that the ſame artifice has been uſed with ſucceſs to ſplit rocks, an operation which is commonly performed by the elaſtic power of gun-powder. A quantity of motion is generated in this experiment, which cannot be referred to the action of gravity. We muſt therefore aſcribe it to another force, namely, the mutual attraction of wood and water in a liquid form.

Aqueous vapour attracted by dry vegetables, &c;

Exp. 2. If a piece of whip-cord or tharm be ſtretched by the heavieſt weight, it can ſupport, the pendent body will aſcend, as oft as the ſtring or gut contracts, in conſequence of an acceſſion of water derived from the atmoſphere; on the contrary, it will deſcend, when the cord begins to relax from the loſs of moiſture. The motion generated in this inſtance, proves atmoſpherical vapour to be powerfully attracted by the dry fibres of vegetables and animals; conſequently theſe ſubſtances have a ſtrong affinity to water, not only in a liquid form, but alſo when it is diffuſed through the air. This affinity or force will be called the hygrometrical attraction in the ſequel of the eſſay, for the ſake of perſpicuity.

The force of affinity permanent.

The preceding experiments have not the leaſt claim to novelty; but they are the preliminaries of an inference, which is of moment in the preſent queſtion. For affinity is a fixed relation of bodies, creating a diſpoſition to coaleſce, in ſuch as are thus mutually related, as often as water is combined with another ſubſtance. The union muſt therefore be permanent, unleſs it happens to be diſſolved by an external cauſe. Now as any certain force only gives way to another ſuperior and contrary to itſelf, it is evident that a moiſt body, which diſcharges a portion of the water it contains, is obliged to part with it by a more powerful attraction, exiſting in its neighbourhood. It is to be remarked, that temperature is one of the external cauſes alluded to above; but it is diſregarded at preſent, becauſe the effects of its changes may be obviated in the following experiment, which is intended to throw additional light upon the hygrometrical attraction.

Hygrometrical attraction is diminiſhed by the acceſſion of water.

Exp. 3. Take two bibulous ſubſtances, ſuch as two ſlices of ſponge, or a piece of ſponge and ſhred of woollen cloth. Make the one wet and keep the other dry; then put them both into a cloſe veſſel of glaſs or metal, placing them either in contact or apart: the wet body will grow lighter in a ſhort time, and the dry one will gain more weight; this proceſs may be prolonged, until the two ſubſtances find the equilibrium of their attractive powers; which will be accompliſhed, when their reſpective weights become ſtationary. This equilibrium proves the hygrometrical attraction to diminiſh with the abſorption of water, and to increaſe with the loſs of the ſame; conſequently an union produced by this force, may be diſſolved by the preſence of a body, which contains leſs water, and therefore attracts it more powerfully.

Atmoſpherical air poſſeſſes this attraction.

Atmoſpherical air may be concluded to poſſeſs the power deſcribed above, from the changes and effects, which are obſervable in the following inſtances: Firſt, Atmoſpherical air takes the water of cryſtallization from various ſalts; it therefore overcomes the affinity, which unites the component parts of theſe cryſtals. Secondly, The ſame hygrometer denotes a greater degree of humidity at one time, than at another, though the height of the thermometer be the ſame; conſequently the hygrometrical attraction of the atmoſphere is variable under equal degrees of temperature; becauſe this force is evidently conſtant in an inſtrument kept in an uniform heat. Third, If two veſſels be expoſed, at the ſame time to the air, one of which contains dry potaſh, and the other a dilute ſolution of the ſame; the former will acquire weight, while the other grows lighter. The laſt fact ſhows, that atmoſpherical air may be ſaturated with moiſture, in reſpect of one body, and be at the ſame time in a very different ſituation relative to another; ſo that evaporation evidently ariſes from an exceſs of hygrometrical attraction in the atmoſphere; on the contrary, the production of dew depends upon a ſimilar exceſs in the bodies on which it is formed.

Atmoſpherical air a ſimple gas,

I may be aſked, after making this open declaration of my ſentiments, which of the conſtituent gaſes of the atmoſphere combine with water? The proper reply to the queſtion appears to be this: It is atmoſpherical air; which I conſider to be a homogeneous gas, for the following reaſons:—Firſt, The atmoſphere is diaphanous; which could hardly be the caſe, were it a maſs of uncombined fluids of different ſpecific gravities; for, had ſuch an arrangement been formed, the rays of the ſun would have ſuffered a multiplicity of refractions, in their approach to the earth; and total darkneſs, or at beſt a dim twilight, would have been the conſequence, had our planet been ſhrouded by a covering of heterogeneous gaſes. Thus the atmoſphere appears to be homogeneous, from the conſideration of its tranſparency.—Secondly, A given meaſure of oxigen is heavier than an equal bulk of azote, under ſimilar circumſtances;

becauſe it is diaphanous,

becauſe ſmart reports are not double to ſenſe.

Atmoſpherical air a ſimple gas.

circumſtances; conſequently the denſity of the former exceeds that of the latter, ſuppoſing their elaſtic forces to be equal. On this account ſounds will move, in all caſes, with leſs celerity in oxigen than they do in azote. If then our atmoſphere conſiſted of two independent maſſes of theſe fluids, mutually pervading each other, every momentary report would have been double to ſenſe, at a ſufficient diſtance from the ſeat of ſound; becauſe ſuch a report would arrive at the ear more expeditiouſly through the medium of the azote, than it would through that of the oxigen. But ſounds of the ſhorteſt duration are not repeated at the greateſt diſtances; conſequently the air is homogeneous, becauſe it is the vehicle of ſound.

If the preceding arguments be juſt, the homogeneity of atmoſpherical air cannot be controverted; becauſe the concluſions which reſult from the contrary hypotheſis are repugnant to common experience. We come in the next place to the ſpecific nature of this gas; but this is a difficult enquiry in the preſent unſettled ſtate of chemiſtry, when the phenomena of galvaniſm are daily bringing new truths to light, and threaten to ſubvert the prevailing theory. Conjectures, however, will naturally ſpring up in the midſt of uncertainty; and as a diverſity of ſentiment has its uſe in times of ſcientific anarchy, I will venture to propoſe the following hypothetical queſtions relative to the conſtitution of common air, Is not this fluid a chemical compound, having the gas called azote for its baſis; to which the poſitive energy of the galvanic pile is united, together with water, but in a manner which diſtinguiſhes this compound from the gaſeous oxide of azote? May not a gas thus conſtituted, oxidate other ſubſtances through the interpoſition of the water, which it holds in ſolution by the hygrometrical attraction? Though the aqueous part of the atmoſphere cannot of itſelf decompound common air; may not it perform the office of an intermediate agent, when aſſiſted by the body to be oxidated, and in this manner deprive the azote of the galvanic energy, more or leſs perfectly, according to circumſtances? Will not the aqueous vapour unite with the matter ſeparated from the air, and produce oxigen gas, which will enter into compoſition with the third ſubſtance, and complete the buſineſs of oxidation? The hints ſuggeſted in the preceding queries, would have been by no means admiſſible in a time of more perfect uniformity in the ſentiments of philoſophers;

loſophers; and nothing can be pleaded in its excuſe but the revolution of opinion, which is apparently ready to take place in the theory of gaſeous fluids.

IV.

Reply to the Obſervations of M. l'Abbé Hauy, on arſeniated Copper. By M. LE COMTE DE BOURNON, *Member of the Royal and Linnean Societies of London.**

Introductory remarks.

It is but a few days, Sir, ſince I had the honour to receive from you the obſervations which you have made on the different ſpecies of arſeniated copper, deſcribed by me, in a memoir on that ſubject, read to the Royal Society of London, on the 19th of February, 1801. I have read theſe obſervations with the greateſt intereſt, but not being able to adopt the opinion reſpecting them, to which the inveſtigations you have ſubmitted them have led you, I feel great obligation for the opportunity you have afforded me of explaining myſelf more particularly than I have hitherto done, on what relates to this intereſting ſubject. Beſides, you offer theſe obſervations with that diffidence which uſually characterizes real merit, accompanied with a doubt which calls for a new examination.

You oppoſe my opinion, Sir, on the diviſion which I have made of the arſeniated copper into four ſpecies, with a delicacy and a politeneſs, which renders the ſlight mineralogical diſcuſſion that becomes the neceſſary reſult of it, of infinite value to me. It is very deſirable that thoſe facts on which differences of opinion may prevail ſhould always be diſcuſſed in this manner: the ſciences would certainly gain by it, and thoſe who cultivate them would loſe nothing by yielding a little to each other.

Notice of the former enquiries.

Like you, Sir, when I employed myſelf on the ſubſtance which, ſince the firſt eſſays of the celebrated Klaproth on it, has been called a combination of the arſenical acid and copper, I thought it right to conſider, under the ſame point of view, the different cryſtalline forms which it offered, deriving them all from one common baſe, and my firſt enquiries were directed to determining this baſe, or the primitive generating cryſtal of all thoſe of this ſubſtance. I was not long in diſcovering that among the cryſtals which I had ſubjected to this examination, there exiſted two forms which could not, in any way, be connected with the others: analyſis has ſince ſhown, that one of theſe belonged to an arſeniated iron, which had been improperly cited as belonging to copper, and the other to a combination of copper and iron with the arſenical acid, which had not been known before. With reſpect to the other cryſtals, as the appearance offered by each of them contradicted the opinion which connected them, it became neceſſary to depend in the beſt poſſible manner on all the other exterior characters which this ſubſtance could offer to the mineralogiſt, to attain to ſome reſult reſpecting it. This is preciſely what I have done, and when the aggregate of theſe characters forced me to recognize four very diſtinct ſpecies in the maſs of ſubſtances, which I ſuſpected might belong to the combination of the arſenical acid and copper, I confeſs I experienced ſome ſatisfaction in obſerving that the analyſis of a chemiſt, ſo juſtly eſteemed as Mr. Chenevix is, ſanctioned, in ſome meaſure, the diviſion to which obſervation had led me. You remark, Sir, that theſe analyſis, on being repeated by M. Vauquelin, varied in their reſult: it follows neceſſarily, that this ſupport fails, or at leaſt becomes uncertain for me: I abandon it therefore, and leave to chemiſtry the diſcuſſion of a fact which belongs to it, and was to me only a powerful auxiliary, to confine myſelf within the ſtrict limits of mineralogy properly ſo called.

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* Tranſlated from the original, communicated by the author. For the paper of the Abbé, ſee p. 187 of our preſent vol.

Preliminary obſervations on the mode of conducting mineralogical enquiries.

But allow me, Sir, firſt to make ſome previous obſervations on the method which, it appears to me, ſhould be followed to determine the union or ſeparation of ſubſtances, and afterwards, on the poſſibility of finding ſeveral ſpecies placed under the combination of the ſame acid with the ſame baſe, but without doubt, having eſſential differences in the manner of combination.

The methods to be employed by the mineralogiſt in the ſtudy of mineral ſubſtances, are comprized in the examination of the peculiar marks which nature has impreſſed on each of the individuals which decorate and enrich its boſom, and which his great habit of obſerving has taught him to recognize. Of theſe marks, which we deſignate by the expreſſion of exterior ſpecific characters, ſome are too delicate to be deſcribed; but cuſtom enables the naturaliſt to ſeize them; their action on his ſight is ſudden; the moſt rapid glance embraces the whole of them, and the naturaliſt has frequently formed his opinion long before he has thought of accounting to himſelf for it. He is not, however, ſecured by them from the errors which other bulkier and more comparable characters may afterwards rectify; but the firſt impreſſion received from theſe firſt traces, very often ſerves him as a guide in the method of employing the ſecond. Among theſe latter characters, ſome are of eaſy application and almoſt always poſſible, others require attention and particular circumſtances to be capable of being employed. Thoſe which are in moſt common uſe, and eaſieſt, are the form, the fracture, the hardneſs, the ſpecific gravity, and the colour. Perhaps in a ſkilful hand, directed by the habit acquired from their uſe, theſe characters are almoſt always ſufficient for the knowledge and claſſification of mineral ſubſtances. In ſtones, the colour is the moſt variable of all: nevertheleſs, it is certain, though the true cauſe cannot yet be aſſigned, that each of thoſe which have been examined hitherto, affects one only of the known colours more readily than it does any of the others. But in the metals this character becomes more conſtant and more eſſential, and it very ſeldom varies without the cauſe of its variation being a change in the nature of the metallic ſubſtance itſelf.

The conſideration of ſome of the characters may be omitted when they do not lead to general inferences.

This fact granted, when the naturaliſt employs the exterior ſpecific characters, to aſcertain the ſubject which determines his enquiry, from the moment at which the agreement of theſe characters, or their differences with thoſe ſhown by known ſubſtances, puts him in a ſituation to pronounce on the identity or the difference of their nature, do not you believe that he has then the liberty of retrenching, on the one hand, thoſe which do not agree with the opinion which he had previouſly thought it right to embrace; and, in the ſecond place, to ſubject the others to ſuppoſitions which may occaſion a change in their aſpect to connect them with that which he wiſhes, when nature itſelf has not offered traces, free from doubt, of the probability of the modification which he admits in theſe characters?

This has been done by M. Hauy.

Permit me to obſerve to you, Sir, that this is preciſely what appears to me to be the ſubſtance of your obſervations on the arſeniated copper. You ſeem to conſider as nothing the very ſenſible differences which exiſt in the divers ſpecies which I have eſtabliſhed, with reſpect to hardneſs, ſpecific gravity, and colour; and, ſtopping at the ſingle character of form, you make ſuppoſitions for each of them, which, in fact, terminate by connecting all thoſe which they offer with one primitive cryſtal: but nature does not exhibit any of the decrements which you ſuppoſe. I have never diſcovered the ſlighteſt trace of them in any of the immenſe quantity of cryſtals of arſeniated copper which have paſſed through my hands. Do you believe that theſe ſuppoſitions are only ſuſceptible of being admitted in a caſe in which, all the other characters being agreed in the moſt perfect ſtate of theſe ſubſtances, which is that of regular cryſtallization and tranſparency, they would become neceſſary only to add an accumulation of proofs to thoſe already acquired of their identity.

Exact diſtinction of the ſpecies neceſſary to the progreſs of the ſcience.

Never was more attention paid than at this moment to the great truth, that the progreſs of the ſciences which lead to the ſtudy of nature, depends principally on the exact diſtinction of each of the ſpecies whoſe union forms the aggregate to which the ſcience is applied. No one is more convinced of this important truth than I am. But this exact knowledge of the ſpecies, which perhaps your calculation or the analyſis of improved chemiſtry may one day attain in a ſimple and accurate manner, reſts at preſent on the agreement of the exterior ſpecific characters. Whenever this agreement exiſts, we are compelled to conclude that there is a ſimilitude in the ſpecies, and, on the contrary, a diſſimilarity when they differ eſſentially from each other. I, however, agree perfectly with you, that before ſeparating one of theſe ſubſtances from the other to make a ſpecies of each, it is requiſite to be previouſly convinced that the differences which they offer, and on which their diviſion reſts, are not purely accidental. It appears to me, therefore, that nothing can be more undeſerving of the reproach of having neglected theſe precautions, than, on the contrary, the eſtabliſhment of the diviſion on the invariable conſtancy in the difference of their exterior characters.

Chemical analyſis does not ſuperſede the mineralogical characters.

The only reaſon, which, in the ſubſtances in queſtion, can raiſe any doubt on their difference, is the reſult obtained from them by chemical analyſis, which conſtantly found the arſenical acid combined with the copper in each of them; but if the analyſis had not been poſſible, certainly no naturaliſt would have heſitated to ſeparate them from [illegible] other, according to the exterior characters ſhown by [illegible] them.

Why therefore, becauſe theſe ſubſtances all belong to the combination of the ſame acid with the ſame metal, ſhould there not be found ſeveral ſpecies among them? This, I believe, is a fact which occurs much more frequently than has been hitherto ſuppoſed. Do not all the metals ſhow various inſtances of ſtriking differences in the oxides, in conſequence of that which exiſts in the combination of oxigen with them? The octahedral attractive oxided iron, that which is rhomboidal, that not attractive, are not all theſe ſo many ſpecies? In a memoir which was inſerted in the 75th number of the *Journal des Mines*, I have endeavoured to ſhow that the octahedral ſulphurated iron, and that in cubes, formed two very diſtinct ſpecies, and I do not believe that theſe are the only ones which exiſt in it. How many ſpecies are offered by ſulphurated copper! I am myſelf acquainted with ſix, all perfectly diſtinct and characterized, which I have long intended to give the deſcription of, but my occupations and want of time have not yet permitted me. Finally, have not you yourſelf been compelled to form a particular ſpecies of carbonated lime, of the arragonite, from the difference alone, which exiſts in its exterior ſpecific characters, although chemiſtry can only find carbonic acid and lime in it?

Variations in the ſpecies of the ſame chemical combination.

One reaſon which may be alledged againſt the diviſion of arſeniated copper into ſpecies, is that the combination of copper with the arſenical acid being already a ſpecies in the genus of ores of copper, it would be making ſpecies of a ſpecies: and this objection, which at the firſt bluſh appears well founded, would bear equally againſt the various oxides, ſulphurets, &c. But this difficulty ſeems to me to be more ſpecious than ſolid: it takes its riſe from the impoſſibility in which we ſtill are of aſcertaining every thing connected with the different cauſes which may produce a variation of the ſpecies. Without doubt, in this inſtance, for example, it is not the ſimple combination of the arſenical acid with the copper which forms the ſpecies, but the particular combination of this acid with the metal. Thus it is not the ſimple combination of oxigen, hidrogen, carbon, azote, &c. which conſtitutes the particular ſpecies of animal, but the manner of the combination itſelf.

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Mineralogy is not yet ſufficiently perfect to admit of the ſeparation of the ſpecies by the form of the primitive moleculæ alone.

The mineralogical ſpecies are very accurately determined by the agreement or difference which exiſts in the firſt moleculæ of the formation of the different ſubſtances, but until we have fixed data to appreciate, in a determinate and invariable manner, all which relates to theſe moleculæ, the conſtancy or the difference in theſe exterior ſpecific characters will always be the only means within our power of uniting or ſeparating the ſpecies. I acknowledge, however, that in this caſe, it is neceſſary that this diviſion ſhould be eſtabliſhed as much as poſſible on ſtriking and eſſential characters; and I agree, at the ſame time, that latterly, this method has perhaps been much abuſed by giving importance to ſimple and caſual characters; this has frequently placed ſubſtances in the number of ſpecies, which ſhould only have been conſidered as ſimple varieties of thoſe already known.

I ſhall now beg of you, Sir, to compare with me the different ſpecies of arſeniated copper which I have deſcribed.

Compariſon of the Firſt and Second Species.

Compariſon of the firſt and ſecond ſpecies of arſeniated copper.

The form of the firſt ſpecies is an obtuſe rectangular octahedron, whoſe faces are unequally inclined. Two of them meet at the ſummit under an angle of 139° and at the baſe under one of 50°. The two others meet at the ſummit in an angle of 115°, and at the baſe in one of 65°. This octahedron is uſually cuneiform: I have never perceived any modification of it.

The form of the ſecond ſpecies is a hexahedral plate, always very thin, whoſe vertical planes are inclined alternately in oppoſite directions, ſo that two of them, on the ſame ſide, make an angle of 135° with the terminal faces to which they incline, and the third, one of 115°.

The moſt uſual colour of the firſt ſpecies is a deep and very brilliant ſky-blue, which ſometimes changes to green.

That of the ſecond ſpecies is a fine emerald green: I have never ſeen any other.

The ſpecific gravity of the firſt ſpecies is 2881: that of the ſecond 2548.

The hardneſs of the firſt is ſuch that it readily cuts carbonated lime: the ſecond is no harder than is ſufficient to cut gypſum.

In your obſervations, Sir, you have raiſed to 50° 4′ and 65° 8′ the meaſures which I had eſtabliſhed at 50° and 65°; meaſures which you have fixed from the relations eſtabliſhed by you between the height of one of the pyramids and the perpendiculars drawn from its bottom on the edges of its baſe, which correſponds with the adjacent and unequally inclined, pyramidal faces. Theſe meaſures are ſo near to mine, that I have proved them again, and the inſtrument is ſo little capable of marking this difference, that I do not make any difficulty in adopting them.

To connect the form of the ſecond ſpecies with the obtuſe octahedron of the firſt, you afterwards ſuppoſe two ſections made parallel to one of the moſt inclined faces of the octahedron, ſo as to detach a very thin ſegment, in which the centre of this octahedron is to be contained; that is to ſay, this octahedron is to be conſiderably increaſed on all its faces, with the exception of one alone, taken on each pyramid, and in an oppoſite direction in each of them. You ſuppoſe at the ſame time a decrement of a ſingle row along the edges of the baſe, but which acts only on two of the faces of the octahedron, and that the ſegment which reſults on two of the three ſides inclined to each of the terminal faces, makes with them an angle of 130½° and the third 115°, meaſures which only differ 5° 30′ in the angle of 130° 30′ from thoſe which I have given for the cryſtal.

The following is the anſwer dictated by the new examination which I have made of this ſubſtance.

Objections to their ſeparation refuted.

Agreeably to what I have ſaid in my memoir on the arſeniated coppers, the obtuſe octahedron frequently ſhows ſlight ſtreaks in its faces parallel to its edges, which indicates a lamellated texture in the direction of theſe faces. The fracture alſo indicates the ſame texture, but its fractures are always more or leſs irregular. I have never been able to obtain a clear one. In the ſecond ſpecies, on the contrary, the laminae are as eaſily raiſed from the hexahedral terminal faces, as could have been done on a priſm of mica. Theſe terminal faces are ſometimes ſtreaked parallel to the edges of the ſides which are inclined to them, and theſe ſtreaks, which are continued ſtrongly on the ſides, never appear upon them except in this direction. This texture, very analogous to that of mica, ſeems to me to be totally different from that of the obtuſe octahedron of the firſt ſpecies.

I have ſubmitted ſome new cryſtals of this ſpecies to meaſurement, and have found them agree perfectly in the meaſures of 115° and 135° with thoſe which I had examined before. The angle ſuppoſed by you of 130° 30′, which I have tried on a number of cryſtals always appeared to me to be much too ſmall. Theſe cryſtals have offered me a new variety, in which the ſides of the hexahedral laminæ are leſs inclined on the terminal faces, with which they form an angle of about 105°. The cryſtal which afforded me this new variety is four lines in diameter: it is only in perfect preſervation in one of its halves, but it admits of a judgment from it, that all its ſides muſt have the ſame inclination. Theſe new faces are perfectly ſmooth, and do not ſhow any ſtriæ. On another cryſtal, inſtead of theſe inclined ſides, two planes are obſerved, one of which belongs to that which made an angle of 105° with the terminal face to which it inclines, and the other belongs either to that of 115° or that of 135°.

A new variety of the ſecond ſpecies.

There was not any thing that I could diſcover connected with any of the planes of the obtuſe octahedron of the firſt ſpecies.

Compariſon of the Third Species with the Firſt.

Compariſon of the third ſpecies with the firſt.

The colour of the firſt is either a deep ſky-blue or a graſs-green. That moſt uſual in the third is a yellowiſh green, more or leſs deep, but very frequently it can only be perceived by placing the cryſtal between the eye and the light, the intenſity of the colour making the cryſtals appear black in every other poſition.

The ſpecific gravity of the firſt is 2381; that of the third 4280.

The hardneſs of the firſt is not more than ſufficient to ſcratch carbonated lime; that of the third is ſuch as to cut fluated lime.

The firſt ſpecies has an obtuſe rectangular octahedron for a unique and primitive cryſtal, whoſe dimenſions have been given above: the figure of the third is an acute rectangular octahedron, in which each pyramid has two faces more inclined than the other two; the two moſt inclined faces meet at the ſummit, in an angle of 84°, and at the baſe in one of 96°, and the two others meet at the ſummit in an angle of 68°, and at the baſe in one of 102°. This octahedron is moſt uſually cuneiform, and its prolongation is ſometimes very conſiderable;

derable; it then takes the appearance of a rhomboidal tetrahedral priſm of 84° and 96°, terminated at its extremities by a dihedral ſummit with iſoſceles triangular planes, the ſummit of which is placed on the edges of 84°, and the baſes meet with each other in an angle of 112°. This form has not hitherto ſhown any other modification except being replaced by a plane, larger or ſmaller, on the edges of 96°. Its planes are uſually very ſmooth and brilliant, and I have never been able to diſcover an appearance of diviſion *(clivage)* in any of them.

This third ſpecies paſſes by the greatly lengthened octahedron to the determinate capillary variety, as well as to that which is indeterminate, and in this caſe the colour appears either to tend more to green or to take a more defined yellow, which ſometimes has the brilliancy of gold.

The firſt ſpecies does not exhibit any thing which reſembles theſe various tranſitions; it is always the ſame obtuſe octahedron, and only varies by a very ſlight prolongation of its cryſtals parallel to the leaſt inclined faces. To make the formation of the acute octahedron of the third ſpecies, ſecondary to the obtuſe one of the firſt, you ſuppoſe a decrement at the baſe of the latter of two rows above and below the edges of the union of the leaſt inclined faces, and another of four rows at that of the union of the moſt inclined faces, and by this, you get an acute octahedron, whoſe moſt inclined faces meet at the ſummit, in an angle of 86° 24′ and at the baſe in an angle of 93° 36′; and the others meet at the ſummit in an angle of 71°, and at the baſe in one of 109°.

I acknowledge that this approximation to the meaſures which I have given is ſeducing, and, conſidering the natural ſmallneſs of the cryſtals of this ſpecies, it would perhaps be difficult for me to pronounce determinately whether the meaſures which I have taken are much more exact than thoſe to which you have attained by calculation; but this I can aſſure you, that no indication whatever, in either of theſe two octahedra, leads to the ſuppoſition which has given you this reſult.

Reaſons for adhering to this diviſion of the ſpecies.

From the details I have now given, it is eaſy to deduce the reaſons which impel me to adhere to the diviſion which I have thought it right to make in the arſeniated copper, and prevent me from adopting the approximation to which your ingenious hypotheſes have led you. Every thing ſtill ſeems to me to tend to indicate a difference in the ſpecies into which I have ſeparated them, while to bring them to a ſingle one, you have been obliged to conſider as nothing, all the exterior ſpecific characters, with the exception of the form alone, and you have uſed the latter only in eſtabliſhing an hypotheſis reſpecting it, to which, neither artificial means, ſuch as ſplitting, nor natural indications, ſuch as ſecondary planes on a primitive cryſtal, nor the retaining of primitive planes on the others have led you. If in the explanation of embaraſſing facts, it were permitted thus to make nature ſpeak when ſhe is ſilent, I have no heſitation in aſſerting that a naturaliſt ſo well informed, and ſo practiſed in the art of calculation as you are, would find very few obſtacles in reſolving all the ſpecies into each other whenever he choſe.

Of the four ſpecies of arſeniated copper which I have deſcribed, there ſtill remains one, in reſpect of which you have not made any calculation of approximation; it is the fourth, which, as I have ſtated, has for a primitive cryſtal a tetrahedral priſm with an equilateral triangle for a baſe. Nevertheleſs you do not exclude it when you draw your concluſions on the doubt which you believe to exiſt on the diviſion of the arſeniated coppers into four ſpecies, and you direct this doubt equally to the fourth. Since according to your ſuppoſition this can only be as a primitive to the firſt cryſtal, that it may alſo be in a ſtate to be brought to it, I have thought it right to add likewiſe the compariſon of this fourth ſpecies with the firſt.

Compariſon of the Fourth Species with the Firſt.

Compariſon of the fourth ſpecies with the firſt.

The colour of the firſt ſpecies is a deep ſky-blue, which ſometimes changes to graſs-green. That of the fourth is a brilliant deep verdigris, but its ſurface is very readily diſcoloured, doubtleſs by oxidating, and it then becomes black: this renders the cryſtals opaque, which, when they have not experienced this alteration, are beautifully tranſparent; this is very unuſual among thoſe which have been naturally expoſed to the free air during a certain time. This change however exiſts only at the ſurface; by ſcratching the cryſtals lightly, their fine colour is readily reſtored to them. I have never perceived any thing reſembling this fact, which unqueſtionably depends on the nature of the ſubſtance of this ſpecies, either among the cryſtals of the firſt, or among thoſe of the ſecond and third.

The ſpecific gravity of the fourth ſpecies is 4280, and is conſequently perfectly analogous to that of the arſeniated copper of the third ſpecies, but at the ſame time greatly inferior to that of the firſt, which is 2881.

Its hardneſs, much below that of the third ſpecies which we have juſt ſeen that it reſembles in weight, is alſo leſs than that of the firſt ſpecies, by which it is cut.

Its forms, which are greatly multiplied, while there exiſts but one in the firſt ſpecies, differ eſſentially from thoſe of the firſt.

Reaſons for forming a fifth ſpecies.

All theſe forms appear to me to be derived from the right tetrahedral priſm with equilateral triangles for baſes; and all thoſe which I have endeavoured to recognize and have given in my memoir, ſeemed to me to be very readily derived from that, as I have alſo ſaid: theſe cryſtals are always extremely ſmall, and I was unable to meaſure them; a few groups of them exhibited this priſm in ſuch a manner that I could obſerve it perfectly complete. The ſcarceſt cryſtals, after thoſe belonging to the varieties which I have repreſented at figures* 15, 16, and 17, and which are uſually ſo grouped as to penetrate each other, and thus to become very difficult to be known, are the very acute complete rhomboid and its incomplete varieties, ſuch as are repreſented at figures 22, 23 and 24 of my memoir. I even heſitated, the diviſion *(clivage)* not having ſhown any thing to guide me, whether I ſhould not take this rhomboid for the primitive form. In this inſtance, the only manner which ſeemed to me natural and ſimple of connecting this cryſtal with the obtuſe octahedron of the third ſpecies, is to ſuppoſe this octahedron become rhomboidal by an increaſe having taken place by the ſuperpoſition of laminæ, or by the collection of rows of moleculæ, growing ſmaller, on only one of the faces of each pyramid, and in an oppoſite direction on each of them, as occurs in the ſpinel, and a number of ſubſtances having a right octahedron for a primitive cryſtal, and which I have alſo ſeen in the diamond. But in this caſe, either the increaſe muſt be made on the moſt inclined faces, and then calculation ſhows that the planes of the rhomboid ſhould have 57° 39′ and 122° 21′ for the meaſure of the angles of its plane; or the ſame increaſe muſt be made on the leaſt inclined faces, and then the meaſures of the angles of the plane of the rhomboid

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* Phil. Tranſ.

Reaſons for forming a fifth ſpecies.

ſhould be 47° 4′ and 132° 56′: now although from its minuteneſs, the rhomboid which exiſts in this ſpecies is not meaſurable by inſtruments, yet I can affirm with certainty that it is much more acute than either of the two which this ſuppoſition gives riſe to.

It is very true, Sir, that ſince my memoir on the arſeniated copper was printed, I have thought it right to ſeparate the ſpecies deſcribed in it, and to make a fifth of one of the ſubſtances included among them; a ſubſtance which is entirely different from the others in its exterior characters, and ſeems to lead naturally to a belief that water muſt be a principal in the number of its component parts: but you are in an error with reſpect to which of theſe ſubſtances I believe to be really of a different nature from the others. In my firſt work on the arſeniated copper, I found it neceſſary to make ſeveral ſubdiviſions or varieties in the fourth ſpecies: of theſe, the three firſt are the determinate capillary, the indeterminate capillary, and that which is ſolid at one of its extremities, and divided into very delicate fibres at the other. You ſeem to believe that I comprehend theſe three varieties in the new ſpecies which I have been led to conſider as an hidro-arſeniate. This opinion would indeed be, as you very juſtly obſerve, completely contradictory of every thing which I have ſaid on theſe varieties, in the deſcription I have given of them, and I have no doubt, muſt have aſtoniſhed you. The only ſpecies of arſeniated copper to which this opinion is directed, is that which includes the two varieties to which I have given the names of hematiform and amiantiform: they are very certainly the ſame, with this difference, that one is the product of the decompoſition of the other.

This arſeniated copper, when it is unmixed, forms very compact mamellæ, but ſtriated from the centre to the circumference, and very often alſo forming different concentric layers: their colour is brown, ſometimes with a very ſlight tinge of green. This ſubſtance, in its aſpect, greatly reſembles the hematiform oxide of tin, which, in *Cornwall*, bears the name of *wood-tin*; this has cauſed the miners of the ſame country to give the name of *wood-copper* to this ſpecies of arſeniated copper. Its hardneſs, notwithſtanding its fibrous texture, is ſufficiently great to ſcratch fluated lime with facility. Its ſpecific gravity is from 4100 to 4200.

This

Reaſons for forming a fifth ſpecies.

This ſubſtance very readily changes; it then paſſes to an aſh-grey, and loſes conſiderably of its hardneſs: it alſo frequently experiences a more advanced decompoſition, and then becomes perfectly white; and ſo ſoft that the nail is ſufficient to cut it and ſeparate its fibres. If the mamellæ which have paſſed to this ſtate are broken, their decompoſition is frequently found to have reached their centre; but very frequently alſo the centre has perfectly retained its brown colour and its hardneſs, and both are obſerved to diminiſh gradually on approaching the circumference; if in this caſe the attention is directed to the fibres near the circumference, it will be ſeen that they are detached from each other, and that the ſurface itſelf of theſe mamellæ has the appearance of that of madrepore, from the immenſe number of fiſſures in different directions, which are occaſioned by the contraction. At length this ſubſtance arrives at ſuch a degree of decompoſition, that the mamellæ open completely, their fibres are entirely ſeparated from each other, and, in this ſtate, frequently become ſo ſlender and flexible as perfectly to reſemble ſmall portions of papyraceous amianthus.

Such, Sir, is the nature of the arſeniated copper, in which I believe I have found properties and a mode of exiſtence which differ from thoſe of the others, and which I have had opportunities of examining with greater facility and accuracy ſince the impreſſion of my memoir. This eaſy decompoſition, the prodigious contraction, and the great change which this ſubſtance experiences in itſelf, have led me to ſuppoſe that the loſs of water has great influence on it. But this fact is, however, nothing more than an opinion, which experience will either reject or confirm; and I have ſo ſtated it. It may alſo be very poſſible that this arſeniate is no more than a variety of the third ſpecies, as I at firſt conſidered it. You muſt, however, acknowledge that, it offers very ſingular characters, of which it would, therefore, be intereſting to know the cauſe.

The dealers in minerals from London, and principally Mr. Mawe, have, I believe, taken a collection of arſeniated copper to Paris. This ſubſtance appears to me worthy of engaging the attention of chemiſts, who, I am of opinion, ſhould repeat the analyſes of it. Perhaps they may one day throw a clearer light on a ſubſtance which has greatly intereſted me, and to which I am at this moment indebted for the pleaſure of having entered with you, Sir, into a diſcuſſion, agreeable in its manner and inſtructive in its effect.

V.

Proceſs for ſeparating Alumine from Alum. In anſwer to the Enquiries of R. T. *By Mr.* FREDERICK ACCUM.

To Mr. NICHOLSON.

SIR,

IF you have not received a better anſwer to the queries of your correſpondent R.T. (p. 141) who finds ſo much difficulty in obtaining alumine in a ſtate of purity, &c. I requeſt you will give a place to the following lines in your next. I have endeavoured to be as conciſe as poſſible, and have the honour to be,

Sir,

Your moſt obedient humble ſervant,

FREDERICK ACCUM.

Old Compton-Street, Soho,

July 17, 1804.

Common proceſs for ſeparating alumine from alum, throws down a ſalt, and not a mere earth.

The proceſs in general recommended by ſyſtematic writers for obtaining alumine, (which R. T. adhered to) is abſolutely erroneous, for no pure earth of alum can be expected if a ſolution of alum of commerce be decompoſed by a ſolution of carbonated alcali, and ſubſequent ablution and expulſion of the carbonic acid. For alum of commerce is a triple compound, conſiſting of alumine, potaſh and ſulphuric acid in exceſs. If we attempt to ſaturate this exceſs of acid, by the addition of an alcali, or even by pure alumine, a highly inſoluble ſalt, (ſulphate of alumine) is generated, differing from alum principally in the proportion of its conſtituent parts. When we therefore gradually add (as R. T. did) to a ſolution of alum, a carbonated alcali, the firſt effect of it is, to ſaturate the exceſs of the ſulphuric acid, and the precipitate conſiſts principally of the ſalt, which is inſoluble in water. A farther addition effects inſtantly a decompoſition of part of the ſalt, which, in proportion as it takes place, becomes mixed with the alumine, which is thus covered or defended from the further action of the alcali. This being the caſe, it is obvious that no ſubſequent waſhing can do more than ſeparate the ſulphate of potaſh, and therefore the reſiduum, inſtead of being pure alumine (as R. T. imagines) contains alſo a variable portion of true ſulphate of alumine.

To prove what has been ſtated before, let any quantity of the obtained alumine be heated in contact with charcoal powder; introduce the mixture into a tabulated retort, connected with the pneumatic apparatus, and add to it muriatic or ſulphuric acid; the reſult will be ſulphuretted hidrogen gas in abundance, particularly if heat be applied to the mixture. The production of this gas will become evident on conſidering the philoſophy of the proceſs. By means of the following method, alumine may be obtained in a purer ſtate.

The precipitate or ſuppoſed earth, if heated with charcoal, and then treated with an acid, gives ſulphuretted hidrogen.

Take any quantity of alum of commerce, diſſolve it in four parts of boiling diſtilled water, and mingle this ſolution with liquid ammonia till no further cloudineſs enſues. Then heat the mixture nearly to the boiling point for a few minutes, and transfer it on a filter. In proportion as the fluid paſſes off, pour more water over the precipitate, and continue the ablution till the water runs off taſteleſs. Having done this, let the precipitate while yet in a paſty ſtate, be transferred into a baſon or flaſk, and add to it muriatic acid, in ſmall quantities at a time, until the whole is diſſolved; then evaporate the ſolution till a drop of it, when ſuffered to cool on a plate of glaſs, yields minute cryſtals. If it now be ſuffered to cool, cryſtals of alum will be depoſited. Remove theſe cryſtals, by decanting the fluid, and renew the evaporation, until on further cooling, no more cryſtals are formed. Nothing now but nearly pure alumine remains in ſolution; the potaſh and ſulphuric acid being got rid of, at the expence of a little alumine in the cryſtals. The fluid may therefore be diluted with water, and then decompoſed by liquid ammonia, taking care to add this alcali in exceſs. The precipitate thus obtained, when waſhed and dried, will be alumine in a ſtate of conſiderable purity.

Method of ſeparating purer alumine.

Precipitate by ammonia; and waſh well. Diſſolve in muriatic acid. Evaporate and cryſtallize for a contaminating portion of alum. Then dilute and precipitate again by ammonia.

The alumine thus obtained does *not* yield ſulphuretted hidrogen when heated with charcoal power; it emits *no* odour when breathed upon, it is ſomewhat unctuous to the touch, inſipid, and white as ſnow.

The earth that now falls is conſiderably pure.

VI.

Short Account of Mr. Arthur Woolf's Improvement in the Conſtruction of Steam-Engines.

Diſcovery that heated ſteam may be ſuffered to expand, with a very powerful effect;—

MR. WOOLF founds his improvements on an important diſcovery he has made reſpecting the expanſibility of ſteam when increaſed in temperature beyond the boiling point, or 212° of Fahrenheit's thermometer. It has been aſcertained for ſome time by Mr. Watt, that ſteam acting with the expanſive force of four pounds per ſquare inch againſt [illegible] ſafety-valve expoſed to the atmoſphere, is capable of expanding itſelf to four times the volume it then occupies, and [illegible] be equal to the preſſure of the atmoſphere. Mr. Woolf [illegible] diſcovered and eſtabliſhed by proof, that ſteam of the force of five pounds the ſquare inch, may expand itſelf to five times its volume; that maſſes or quantities of ſteam of the like expanſive force of ſix, ſeven, eight, nine, or ten pounds the ſquare inch, may be ſuffered to expand to ſix, ſeven, eight, nine, or ten times its volume, and will ſtill be reſpectively equal to the atmoſphere, or capable of producing a ſufficient action againſt the piſton of a ſteam-engine to cauſe the ſame to riſe in the old engine (with a counterpoiſe) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines firſt brought into effect by Meſſrs. Boulton and Watt;—that this ratio is progreſſive, and nearly if not quite uniform, ſo that ſteam of the expanſive force of 20, 30, 40, or 50 pounds the ſquare inch of a common ſafety-valve will expand itſelf to 20, 30, 40, or 50 times its volume with like effect; and that, generally, as to all the intermediate or higher degrees of elaſtic force, the number of times which ſteam of any temperature and force can expand itſelf is nearly the ſame as the number of pounds it is able to ſuſtain on a ſquare inch expoſed to the common atmoſpheric preſſure: provided always that the ſpace, or veſſel in which it is allowed to expand itſelf, be of the ſame temperature as that of the ſteam before it was allowed room to expand.

—the increaſe of temperature not being conſiderable.

Reſpecting the different degrees of temperature required to bring ſteam to, and maintain it at, different expanſive forces above the weight of the atmoſphere, Mr. Woolf has found, by actual experiment, ſetting out from the boiling point of water, or 212°, at which degree ſteam of water is only equal to the preſſure of the atmoſphere, that in order to give it an increaſed elaſtic force equal to five pounds the ſquare inch, the temperature muſt be raiſed to about 227½°, when it will have acquired a power to expand itſelf to five times its volume, ſtill being equal to the atmoſphere, and capable of being applied as ſuch in the working of ſteam-engines, according to his invention: and with regard to various other preſſures, temperatures, and expanſive forces of ſteam, the ſame are ſhown in the following table:

Table of the relative preſſures per ſquare inch, temperatures and expanſibility of ſteam at degrees of heat above the boiling point of water, beginning with the temperature of ſteam of an elaſtic force equal to five pounds per ſquare inch, and extending to ſteam able to ſuſtain forty pounds on the ſquare inch.

	Pounds per ſquare Inch.		Degrees of Heat.		Expanſibility.	
Steam of an elaſtic force predominating over the preſſure of the atmoſphere upon a ſafety-valve,	5	requires to be maintained by a temperature equal to about	227½	and at theſe reſpective degrees of heat, ſteam can expand itſelf to about	5	times its volume, and continue equal in elaſticity to the preſſure of the atmoſphere.
	6		230¼		6	
	7		232¾		7	
	8		235¼		8	
	9		237½		9	
	10		239½		10	
	15		250½		15	
	20		259½		20	
	25		267		25	
	30		273		30	
	35		278		35	
	40		282		40	

Table of preſſures, temperatures and expanſions of ſteam equal in force to the atmoſphere.

And ſo in like manner, by ſmall additions of temperature, an expanſive power may be given to ſteam to enable it to expand to 50, 60, 70, 80, 90, 100, 200, 300, or more times its volume, without any limitation but what is impoſed by the frangible nature of every material of which boilers and the other parts of ſteam-engines have been or can be made; and prudence dictates that the expanſive force ſhould never be carried to the utmoſt the materials can bear, but rather be kept conſiderably within that limit.

Upon this diſcovery, Mr. Woolf has obtained a patent for various improvements in the ſteam-engine, from the ſpecification of which, the following extract is made, which will be ſufficiently inſtructive to thoſe who are acquainted with the ſubject.

Extract from Mr Woolf's ſpecification.

"If the engine be conſtructed originally with the intention of adopting my ſaid improvement, it ought to have two ſteam veſſels of different dimenſions, according to the temperature or the expanſive force determined to be communicated to the ſteam made uſe of in working the engine; for the ſmaller ſteam veſſel or cylinder muſt be a meaſure for the larger. For example, if ſteam of forty pounds the ſquare inch is fixed on, then the ſmaller ſteam veſſel ſhould be at leaſt one fortieth part the contents of the larger one; each ſteam veſſel ſhould be furniſhed with a piſton, and the ſmaller cylinder ſhould have a communication both at its top and bottom (top and bottom being here employed merely as relative terms, for the cylinders may be worked in a horizontal or any other required poſition, as well as vertical): the ſmall cylinder, I ſay, ſhould have a communication both at its top and bottom with the boiler which ſupplies the ſteam, which communications, by means of cocks or valves of any conſtruction adapted to the uſe, are to be alternately opened and ſhut during the working of the engine. The top of the ſmall cylinder ſhould have a communication with the bottom of the larger cylinder, and the bottom of the ſmaller one with the top of the larger, with proper means to open and ſhut theſe alternately by cocks, valves, or any other well-known contrivance. And both the top and bottom of the larger cylinder or ſteam veſſel, ſhould, while the engine is at work, communicate alternately with a condenſing veſſel, into which a jet of water is admitted to haſten the condenſation, or the condenſing veſſel may be cooled by any other means calculated to produce that effect. Things being thus arranged, when the engine is at work, ſteam of high temperature is admitted from the boiler to act by its elaſtic force on one ſide of the ſmaller piſton, while the ſteam which had laſt moved it has a communication with the larger ſteam veſſel or cylinder, where it follows the larger piſton now moving towards that end of its cylinder which is open to the condenſing veſſel. Let both piſtons end their ſtroke at one time, and let us now ſuppoſe them both at the top of their reſpective cylinders, ready to deſcend; then the ſteam of forty pounds the ſquare inch entering above the ſmaller piſton will carry it downwards, Extract from Mr. Woolf's ſpecification.

wards, while the ſteam below it, inſtead of being allowed to eſcape into the atmoſphere or applied to any other purpoſe, will paſs into the larger cylinder above its piſton, which will take its downward ſtroke at the ſame time that the piſton of the ſmaller cylinder is doing the ſame thing; and while this goes on, the ſteam which laſt filled the larger cylinder, in the upward ſtroke of the engine, will be paſſing into the condenſer to be condenſed during the downward ſtroke. When the piſtons in the ſmaller and larger cylinder have thus been made to deſcend to the bottom of their reſpective cylinders, then the ſteam from this boiler is to be ſhut off from the top and admitted to the bottom of the ſmaller cylinder, and the communication between the bottom of the ſmaller and the top of the larger cylinder is alſo to be cut off, and the communication to be opened between the top of the ſmaller and the bottom of the larger cylinder; the ſteam, which in the downward ſtroke of the engine filled the larger cylinder, being now open to the condenſer, and the communication between the bottom of the larger cylinder and the condenſer ſhut off; and ſo on alternately, admitting the ſteam to the different ſides of the ſmaller piſton, while the ſteam laſt admitted into the ſmaller cylinder paſſes alternately to the different ſides of the larger piſton in the larger cylinder, the top and bottom of which are made to communicate alternately with the condenſer.

"In an engine working with the improvements which have been juſt deſcribed, while the ſteam is admitted to one ſide of the piſton in the ſmaller cylinder, the ſteam on the other ſide has room made for its admiſſion into the larger cylinder, on one ſide of its piſton, by the condenſation going on on the other ſide of the large piſton which is open to the condenſer; and that waſte of ſteam which takes place in engines worked only by the expanſive force of ſteam, from ſteam paſſing the piſton, is prevented; for all ſteam that paſſes the piſton in the ſmaller cylinder is received into the larger.

"In ſuch an engine, where it may be more convenient for any particular purpoſe, the arrangement may be altered, and the top of the ſmaller made to communicate with the top of the larger, and the bottom of the ſmaller with the bottom of the larger cylinder; in which caſe the only difference will be, that when the piſton in the ſmaller cylinder deſcends, that in the larger will aſcend, and while the latter deſcends the former will aſcend, which for ſome particular purpoſes may be more convenient than the arrangement before deſcribed."

Mr. Woolf then proceeds to deſcribe various other modifications of his invention, and points out means for applying his improvements to the working of ſteam-engines already conſtructed and now in uſe. It is obvious that the advantages of this diſcovery promiſe to be very conſiderable, and I ſhall take the earlieſt opportunity of communicating the facts as they ſhall be brought forward in practice.

VII.

Deſcription of a Chemical Lamp, with double concentric Wicks. Communicated by Mr. Frederic Accum.

Deſcription, &c. of an improved chemical lamp.

THOSE who are familiar with Chemiſtry will readily allow, that one of the principal obſtacles which frequently impede the progreſs of the young chemiſt in the proſecution of his ſcience, is the want of proper apparatus: he is at a loſs to ſelect from the number of inſtruments diſplayed in the lectures of his teacher, thoſe which are calculated for rendering, *in his own apartment*, the moſt important truths of the ſcience legitimate by experiment.

It muſt naturally be ſo, as long as chemiſts are anxious to exhibit a variety of coſtly and complicated apparatus, and continue to pay that frivolous regard to *ſhow* which characteriſes ſo many public lectures; and as long as the ſtudent is told in the introductory leſſon, that the ſcience cannot be learned but in the laboratory, fitted up with furnaces, ſtills, bellows, water-baths, ſand-baths, *&c.* It is true indeed, that many chemical phenomena cannot be accurately obſerved without the help of inſtruments calculated to aſſiſt the imperfection of our ſenſes; but it is equally true, that many of the brilliant apparatus which are daily diſplayed in the laboratories of teaching chemiſts, as inſtruments of reſearch, ſerve more to divert the attention of the auditors, than to elucidate the fundamental truths of the ſcience.

The modern proceſſes of philoſophical enquiry differ ſo much from what they formerly were, and the inſtruments of experiment

Deſcription, &c. of an improved chemical lamp.

ment have been ſo much improved during our own time, that by means of a comparatively ſmall number of them, the moſt complicated proceſſes may very commodiouſly and conveniently be carried on in the cloſet of every cultivator of chemiſtry.

To awaken the induſtry of the junior claſs of chemiſts, I gave in the twenty-fourth number of this Journal, a deſcription and ſketch of a convenient and portable univerſal furnace, and in the twenty-third number of the ſame Journal, I ventured to recommend to the young analyſt a cheap and uſeful apparatus for drying the products of his analyſis, and alſo convenient for experiments on artificial cold, *&c.* The favourable reception which theſe trifles have met with amongſt thoſe amateurs of the ſcience for whom I have cauſed them to be conſtructed, has encouraged me to point out to them an improved lamp-furnace, by means of which, in the ſmall way and on the table of the ſtudent, almoſt every operation may be performed at a cheap rate, as well as with facility and diſpatch. In order to enable the intelligent reader to judge for himſelf, I ſhall firſt detail the conſtruction of the inſtrument, and then point out to the inexperienced operator ſome of the moſt capital proceſſes which ſerve to unfold the fundamental truths of the ſcience, and for which the lamp may be applied according to the conditions laid down.

Fig. 1, *Plate* XIV. is a perſpective view of the improved chemical lamp-furnace. It conſiſts of a braſs rod ſcrewed to a foot of the ſame metal, loaded with lead. On this rod (which may be unſcrewed in the middle, for rendering it more portable) ſlide three braſs ſockets with ſtraight arms, terminating in braſs rings of different diameters. The largeſt meaſures four inches and a half. Theſe rings ſerve for ſupporting glaſs alembics, retorts, Florence-flaſks, evaporating-baſons, gas-bottles, *&c.* for performing diſtillations, digeſtions, ſolutions, evaporations, ſaline fuſions, concentrations, analyſes with the pneumatic apparatus, *&c.* If the veſſels are not wiſhed to be expoſed to the naked fire, a copper ſand-bath may be interpoſed, which is to be previouſly placed in the ring. Each of the braſs-rings may, by means of a thumb-ſcrew acting on the rod of the lamp, be ſet at different heights, or turned aſide, according to the pleaſure of the operator. Below theſe rings is a fountain-lamp on Argand's plan, having a metallic valve

within,

Deſcription, &c. of an improved chemical lamp.

within, to prevent the oil from running out while the reſervoir is put into its place. This lamp alſo ſlides on the main braſs rod by means of a ſocket and thumb-ſcrew. It is therefore eaſy to bring it nearer, or to move it farther, at pleaſure, from the veſſel which may remain fixed; a circumſtance which, independent of the elevation and the depreſſion of the wicks of the lamp, affords the advantage of heating the veſſels by degrees after they are duly placed, as well as of augmenting or diminiſhing the heat inſtantly; or for maintaining it for ſeveral hours at a certain degree, without in the leaſt diſturbing the apparatus ſuſpended over it. It may therefore be uſed for producing the very gentle heat neceſſary for the rectification of ethers, or the ſtrong heat requiſite for diſtilling mercury.

The chief improvement of this lamp conſiſts in its power of affording an intenſe heat by the addition of a ſecond cylinder added to that of the common lamp of Argand. This additional cylinder incloſes a wick of one inch and a half in diameter, and it is by this ingenious contrivance, which was firſt ſuggeſted to me by Mr. Webſter, that a double flame is cauſed, and more than three times the heat of an Argand's lamp of the largeſt ſize is produced. This part of the conſtruction of the lamp is clearly ſhewn in *Fig.* 2, which repreſents the concentric wick-holders of the lamp; the diſtance between the exterior and interior cotton is half an inch, the circumference of the largeſt wick is 4¼ inches, and that of the ſmaller two. Both the wick-frames are connected by a fine ſcrew cut upon a piece of pinion-wire.

Fig. 3. is a ſection of the concentric cylindrical tubes in which the wicks move.

The ſuperior advantages of this lamp, above all others I am acquainted with, conſiſts therefore in quickly producing, if required, a very low as well as intenſe heat, and in regulating its power inſtantaneouſly; by means of which the operator may obſerve a number of minute circumſtances eſſential to be known, but which cannot be noticed when the ſame proceſs is carried on within a furnace.

Uſe of the Lamp-Furnace.

From what has been ſtated it is obvious, that this lamp may be uſed for a variety of chemical operations, if conducted under the conditions here pointed out; a few of which are—

1. For

Deſcription, &c. of an improved chemical lamp.

1. For obtaining oxigen gas, by means of the glaſs retort, from a mixture of four parts of finely powdered black oxide of manganeſe, and three of concentrated ſulphuric acid; or by merely heating in a retort, hyper-oxigenized muriate of potaſh.

2. For diſengaging nitrogen, or azote, from animal ſubſtances, by affuſing two parts of nitric acid of commerce, diluted with an equal bulk of water, upon one part of muſcular fleſh (a piece of beef or veal cut into ſmall pieces), and heating the mixture to ebullition in a glaſs retort.

3. For obtaining hidrogen, ſulphurated and phoſphorated hidrogen, in the uſual manner; or for producing heavy carbonated hidrogen by ſtrongly heating, in a retort, a mixture of three parts, by weight, of concentrated ſulphuric acid, and one of ſulphuric ether, or two of alcohol; nitrous gas, by any of the uſual proceſſes; gazeous oxide of nitrogen, by decompoſing nitrate of ammonia by heat in a retort; ſulphureous acid gas, by cauſing four parts of concentrated ſulphuric acid to act on one of lump-ſugar, aſſiſting the action of the mixture in the retort by heat; muriatic acid gas; oxigenized muriatic acid gas, and carbonic acid gas, according to any of the uſual methods; ammoniacal gas, by heating in a retort a mixture of two parts of finely powdered lime and one of muriate of ammonia.

4. For the diſtillation of nitric, muriatic, oxi-muriatic, acetic, oxalic, arſenic, pruſſic, ſuberic, mucous, and camphoric acids, according to the methods recommended by ſyſtematic writers.

5. For the production of metallic, earthy, and alkaline ſulphurets; ſuch as ſulphuret of potaſh, ſoda, barytes, ſtrontia, ammonia, iron, copper, tin, mercury, &c.

6. For performing the analyſis of ores of gold, ſilver, copper, lead, zinc, tin, &c. and for examining mineral and native ſalts, earth and ſtones, according to the methods pointed out in the "Practical Eſſay on the Analyſis of Minerals," and for a variety of other operations, too numerous to be detailed.

Old Compton-Street, Soho,
July 18, 1801.

VIII.

On the mutual Precipitations of Metallic Oxides. By J. L. Gay-Lussac.*

The ſubject is hitherto but little known.

Utility of the inveſtigation.

THAT we have ſtill ſo little information on the mutual precipitations of metallic oxides, can only be attributed to the complication of the reſults which they offer. Indeed, a little reflection will convince us that the oxidation, the affinity, the reciprocal action of the oxides, and the property which they have of neutralizing the acids unequally, are ſo many cauſes which muſt join in the production of the phenomena. It would, nevertheleſs, be very uſeful to know the order in which the metallic oxides precipitate from their ſolutions: the chemical analyſes, and more eſpecially the purification of metallic ſalts, would thence become eaſier. It was with this intention that I have made ſome experiments; and if they have not been ſufficiently numerous to have enabled me to diſtinguiſh the influence of every cauſe, they, at leaſt, ſhow that of ſome, and will ſerve to draw the attention to a ſubject which is ſtill obſcure and very complicated.

I ſhall begin by relating the reſults which I have obtained, and ſhall afterwards endeavour to determine their cauſes.

Experiments with the green and red muriates of iron.

Having taken a ſolution of green muriate of iron, I added to it a little red muriate of the ſame metal, and I poured potaſh into the mixture in a quantity at leaſt ſufficient to decompoſe all the red muriate ſeparately. After agitation, the firſt portions of alkali yielded a precipitate of iron very much oxided, without any mixture of black oxide; but by adding more and more of the alkali, it finiſhed by being compoſed of the two oxides. The filtered liquor was then perfectly limpid, and no longer produced a blue with the pruſſiates, nor a black with the gallic acid: this proves that the very oxided iron had been precipitated from it. On making the inverſe experiment, that is to ſay, on putting a little green muriate of iron into a larger quantity of red muriate, and precipitating it by the alkali, the black oxide was retained in the ſolution to the laſt, and was not precipitated until after all the red oxide. Hence therefore it reſults that the black oxide

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* From the Annales de Chimie, No. 145. or Vol. XLIX. p. 21.

Black oxide of iron precipitates the red.

of iron precipitates the red oxide, and that it is, conſequently, very eaſy to have green ſolutions of iron without red oxide.

Experiments with the ſulphate of zinc of commerce.

Into a ſolution of ſulphate of zinc of commerce, which is known to contain much iron, I poured a little potaſh, to produce a precipitate, and I agitated and heated the mixture. On examining the precipitate I found oxide of zinc and a little iron very much oxided, and nevertheleſs the liquor ſtill contained much iron, but it was at the minimum of oxidation: an addition of alkali only ſeparated oxide of zinc. I divided the filtered liquor into two portions; into one I poured oxigenated muriatic acid, and I boiled the other with a little nitric acid. Potaſh then poured into the two liquors ſeparated all the iron from them, ſo that there only remained a very pure ſulphate of zinc, containing only a little ſulphate of potaſh, which it is eaſy to avoid by employing oxide of zinc, recently prepared and well waſhed, to ſeparate the iron. On making the ſame experiments on other ſolutions of zinc, I conſtantly found that oxide of zinc precipitated red oxide of iron, and that, on the contrary, it was precipitated by the black oxide.

Oxide of zinc precipitates red oxide of iron, but is precipitated by the black.

A ſolution of zinc in nitric acid may be directly obtained, ſufficiently pure, by diſſolving it very rapidly: great part of the very oxided oxide of iron is precipitated; and that which remains in ſolution requiring a great exceſs of acid, is precipitated by diſſolving another quantity of zinc. But if the ſolution has been made ſlowly, it retains much iron, which being but little oxided, is retained very ſtrongly.

The quantity of alkali employed ſhould be ſmall.

It is well known that, in all experiments of this kind, the precipitate may be compoſed of one or of two oxides, according to the quantity of alkali employed; but the better to obſerve what paſſes, it is adviſeable to put very little alkali into the metallic ſolution, the precipitate being in that caſe compoſed of only one oxide.

Importance of the mutual precipitations of copper and iron.

By continuing to follow the ſame proceſſes I found, that when the iron is very much oxided it is precipitated by oxide of copper, and that the inverſe takes place when it is but little ſo. Here are two very important conſequences, becauſe they may be very frequently applicable in the arts: the firſt is, that all the iron may be ſeparated from a ſolution of copper; the ſecond, that all the copper contained in a green ſolution of iron may be abſtracted.

Sulphate of copper may be freed from iron:

Several colours are prepared with ſulphate of copper, but the iron which it always contains, and which hitherto has never been completely ſeparated, alters the ſhade. If, to accompliſh this laſt object, the iron is ſtrongly oxided by means of nitric acid, or, which is better, of oxigenated muriatic acid, the ſulphate of iron may be intirely precipitated by pouring a ſufficient quantity of potaſh into it, and by heating and agitating the liquor.[*]

and ſulphate of iron from copper.

Green ſulphate of iron is alſo frequently employed in the arts, and in many of them it is deſirable that it ſhould not retain any copper. Iron has the property of ſeparating it, but it appears that it only does ſo imperfectly, and requires much time. It would doubtleſs be more advantageous to employ potaſh, and to pour a little into the green ſulphate: the precipitate of black oxide of iron would be ſpeedily re-diſſolved by agitation, and would precipitate, at the ſame time, the oxide of copper and the red oxide of iron, if there was any in the green ſulphate.

Ammonia diſſolves oxide of iron at a minimum.

I ſhall here remark that, having employed ammonia to diſcover the copper in the green ſulphate, I obſerved that, on adding an exceſs of alkali, the oxide of iron was diſſolved in great abundance, though it is known that, in the ſame circumſtances, it does not diſſolve the very oxided iron. The ſolution left in the air is decompoſed, the ammonia eſcapes, and a black cruſt is formed on the ſurface of the liquid which ſoon defends it from the contact of the air. In analyſes, ammonia is frequently employed to ſeparate iron, but this method is not good unleſs it had been ſtrongly oxided before. This induces me to believe that this circumſtance might have prevented Bergman from ſeparating iron from nickel by means of ammonia; for he found that his ſolution contained the oxides of both metals, and this could only have ariſen from the iron not having been ſufficiently oxided.

Oxigenated muriate of mercury precipitates the red oxide of iron and thoſe of zinc and copper.

I alſo diſcovered, by the ſame means, that the oxide of oxigenated muriate of mercury precipitates the red oxide of iron with the greateſt facility, and thoſe of zinc and copper from their muriatic ſolution.

Oxide of ſilver precipitate. oxide of copper.

Having diſſolved a piece of ſilver in nitric acid, I obtained a blue liquor compoſed of copper and ſilver. A little potaſh poured into the ſolution formed a flocculent precipitate, compoſed,

poſed, in great part, of oxide of ſilver, becauſe the precipitation took place only where the alkali was poured in; but this precipitate was gradually covered with oxide of copper, and, by agitation, it was in a little time replaced by the latter. An addition of alkali having given me a precipitate of oxide of ſilver which was not re-diſſolved, I filtered it, and obtained a liquid perfectly colourleſs, which did not contain any more copper. If it is wiſhed to avoid the potaſh in the ſolution, a part of the impure nitrate of ſilver may be decompoſed ſeparately, and the precipitate, well waſhed, may be employed to ſeparate the copper of the other part. This ſimple method of ſeparating copper from a ſolution of ſilver may be very uſeful in laboratories, and even in large works.

The oxide of ſilver alſo decompoſes the nitrate of zinc; and the oxide of manganeſe the muriate of copper.

The precipitates are not pure oxides.

In what precedes I have, for ſhortneſs, ſuppoſed that the precipitates were pure oxides; but I am far from believing this to be the caſe; on the contrary, I conſider nearly all of them as true ſalts. Copper, for example, was always precipitated of a bluiſh-green, although the ſhade varied with the oxides which were precipitated; and it is now well aſcertained by Proult and the younger Berthollet, that the green and blue oxides of copper retain ſome of the acid.

Such are the facts as I have obſerved them, and on which alone I ſhall make ſome reflections. Although they are too few to have enabled me to fix on all the circumſtances which concurred in their production, the examination of them will, however, develope ſome of them.

Inferences from a conſideration of the ſtate of the acidity.

In fact, if we direct our attention to the acidity of the different ſalts noticed above, we ſhall ſee,

Neutralization of the acids by oxides.

1ſt. That the iron which is but little oxided, and the highly oxided mercury which precipitates the red oxide of iron, the oxide of zinc, and that of copper, approach nearer to neutralization than the latter.

2d. That the zinc and manganeſe which precipitate the copper, neutralize the acids better than it[*].

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[*] By neutralizing the acids more or leſs, I mean that property poſſeſſed by the metallic oxides, and ſome earths, ſuch as glucine and alumine, of approaching more or leſs, in their combinations with the acids, to the term of neutralization.

3*d*. That the oxide of ſilver which precipitates thoſe of zinc and copper, neutralizes the acids better than them.

Alumine is precipitated by metallic oxides;

If, beſides, we reflect, that alumine, whoſe ſolutions are very acid, is precipitated by ſeveral metallic oxides which neutralize the acids better than it; that, according to the experiments of Vauquelin, glucine decompoſes aluminous ſalts, and that its ſolutions are more neutral than thoſe of alumine, although they are not entirely ſo; and, finally, that magneſia, which neutralizes the acids perfectly, precipitates the preceding earths, and a very great number, not to ſay all, the oxides from their ſolutions; we cannot abſtain from allowing that, if the property poſſeſſed by the metallic oxides and ſeveral earths of neutralizing the acids unequally, is not the only cauſe of the decompoſitions which I have detailed, it is at leaſt one of the principal.

and by glucine.

Magneſia precipitates the earths and oxides.

The affinity for oxigen is not the cauſe.

We may alſo conclude from the ſame experiments, that the metals which have a great or a weak affinity for oxigen, do not enjoy any particular property with reſpect to their mutual precipitations; for we ſee that iron, in a ſtate of great oxidation, is precipitated by a number of oxides which it precipitates when it is leſs ſo; and, that there are ſeveral oxides which contain leſs oxigen than that of zinc, which precipitate the latter, while there are others which are precipitated by it.

The affinity of the different metals for oxigen is therefore rejected as the cauſe of the mutual precipitations of their oxides; but can the greater or leſs oxidation of the ſame metal occaſion a variation in the affinity of the oxide for the acids? This opinion has been promulgated by Cit. Berthollet in his Chemical Statics *, and he has grounded it upon ſeveral facts, in which the metal, by loſing a little of its oxigen by any means whatever, forms another ſalt with leſs acid. This happens to the oxigenated muriate of mercury, which, by expoſure to light, or by being brought into contact with iron, is changed into white muriate by abandoning ſome of its acid. Although theſe, and other ſimilar facts, are capable of a different interpretation, other conſiderations, which I ſhall omit here, becauſe they would lead me too far, induce me to participate in the opinion of Citizen Berthollet; but I do not be-

The ſtate of the oxidation influences the affinity of the oxides for the acids;

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* A tranſlation of which will be publiſhed by Mawman, in the Poultry, about the middle of the preſent month.

lieve that this cauſe can have much effect, it being ſtrongly counteracted by the acidity which almoſt all the metallic ſolutions poſſeſs, and by the inſolubility of the oxides. Thus, although it appears to me that iron, when little oxided, has more affinity for muriatic acid than when it is greatly oxided, I ſhould rather attribute the precipitation of the latter by the former to the great exceſs of acid which its ſolution requires, than to its weaker affinity.

but is not the cauſe of their mutual precipitations.

Neither, for the ſame reaſons, do I believe that the affinity of the different oxides for the acids, an affinity which I meaſure, with Cit. Berthollet, by the capacities for ſaturation, can be conſidered as the cauſe of their mutual precipitations.

Influence of the retained acid.

Beſides, there is one conſideration of ſome importance which ſhould be taken into the explanation of the mutual precipitations of the oxides; it is that, in a caſe where the precipitation of a metallic ſolution is produced by means of an alkali, the precipitate retains ſome of the acid which can favour its ſolution; ſo that an oxide which could retain much of the acid, would diſſolve more readily than that which could only retain leſs. It muſt really be ſo with iron, which, when it is precipitated from a green ſolution, retains much more acid than when it is precipitated from a red ſolution, and which diſſolves much better in the acids in the firſt caſe than in the ſecond. This more ready ſolution cannot, however, be conſidered as a cauſe of the mutual precipitations of the metallic oxides; it may be very favourable to them, but cannot determine them. In fact, we ſee that the oxide of copper, which retains much acid, is nevertheleſs precipitated by oxide of ſilver, which does not ſenſibly retain any, when they are precipitated from their nitric ſolutions by potaſh.

CONCLUSION.

General inferences.

THE metallic oxides are mutually precipitated from their ſolutions. Several cauſes may contribute to this; but among the number of the principal muſt be placed the property which they have of neutralizing the acids unequally.

This property has furniſhed us with the means, 1*ſt*. Of freeing a green ſolution of iron from the red oxide which it may contain; 2*d*. Of ſeparating the ſulphate of zinc and that of copper from the iron which is always found in them; 3*d*. Of having a green ſulphate of iron free from copper; 4*th*. Of

General inferences.

readily ſeparating the copper from a ſolution of ſilver. It is eaſy to accumulate theſe applications by extending them to a greater number of ſubſtances. Thus, the oxides of cobalt and nickel do not neutralize the acids equally; that of the two which neutralizes it moſt, will be able to precipitate the other, and remain alone in the ſolution. Thus, alſo, ſince glucine neutralizes the acids much better than highly oxided iron, it will be eaſy to ſeparate this metal from its ſolutions, by firſt oxidating it ſtrongly, and afterwards precipitating one part of the ſolution, to employ it, after being well waſhed, to precipitate the iron of the other part.

The greater or leſs affinity of the metals for oxigen does not give them any particular property with reſpect to the mutual precipitation of their oxides.

Oxidation produces a variation in the affinity, or the capacity for ſaturation of the oxides for the acids; nevertheleſs, the reſults are only ſenſible inaſmuch as they produce a change in the neutralization, and in this caſe they may be attributed to the latter cauſe.

The affinity of the oxides for the acids may indeed contribute to their mutual precipitations, but its effects are very limited.

It appears, therefore, in general, that, all circumſtances remaining otherwiſe the ſame, the ſubſtances which neutralize the acids beſt, may precipitate the others from their ſolutions.

I repeat in concluding, that it is only on the facts which I have related that I have eſtabliſhed my reaſoning, and that it was not my object in this note to ſpeak of the precipitations by the metals, nor of thoſe which are owing to the reciprocal action of the oxides, or to that of the latter and the alkalis.

A Report

IX.

A Report of the State of his Majeſty's Flock of Fine Wooled Spaniſh Sheep, for the year ending Michaelmas, 1803. *By the Right Honourable Sir* JOSEPH BANKS, *Bart. P. R. S.* &c. &c. *From the printed Copy communicated by the Author.**

State of his Majeſty's flock of Spaniſh ſheep, June 1802.

THE wether lambs of the laſt year having been ſold in their wool, and the rams wool retained, in order that two years growth might be prepared for ſale together, his Majeſty's Spaniſh flock conſiſted, when ſhorn in June 1802, of ninety-ſix ewes only; the fleeces of theſe, after having been waſhed on the ſheeps' backs as uſual, weighed as follows:

In wool, as ſhorn from the ſheep - -	352 lbs.
Loſs in ſcowering - - - - -	96
Amount of ſcowered wool - - -	256

This wool, when ſorted, produced as follows:

Prime wool, or R.	221 lbs. at 5*s*. 9*d*.			£.63	10	9
Choice locks, or F.	32 ——	3	6	- 5	12	0
Fribs, or T. -	3 ——	1	9	- 0	5	3
				£.69	8	0

After deducting the expence of ſorting and ſcowering, at the high rate which an individual who is not a manufacturer muſt pay for theſe proceſſes, this wool is worth about 5*l.* a tod, or 43*l.* 5*s.* a pack, as clipped from the ſheeps' back.

Sale of the wool;

The prime wool was purchaſed by John Maitland, Eſq. member of parliament for Chippenham, whoſe mercantile houſe, eſtabliſhed for more than a century, has always dealt largely in the importation of Spaniſh wool, and who from the firſt introduction of Merino ſheep into this country by the King in the year of 1787, has uniformly given the moſt liberal and zealous aid to the promotion of his Majeſty's patriotic views, though doubtful in the beginning of the ultimate ſucceſs of the project.

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* For the original project of this important undertaking and its ſubſequent progreſs, ſee the former reports by the ſame Right Hon. Gentleman, inſerted in our Journal, quarto, vol. IV. p. 289, and octavo, vol. V. p. 65.

It

manufactured into cloth;

It was made into cloth by Mr. Edridge, a manufacturer of Chippenham, whoſe ſkill and reſpectability in his line is exceeded by no man. He inſpected its quality with the moſt minute exactneſs, and with an eye more inclined to expect ſymptoms of degeneration than of improvement, during the whole of the numerous proceſſes to which wool is ſubjected in the making of broad cloth, and he found that in every one of them it anſwered to his complete ſatisfaction.

The cloth made from this wool proved ſo excellent in its kind, that the King was graciouſly pleaſed, at the deſire of Mr. Maitland and Mr. Edridge, to permit theſe gentlemen to explain, in his Majeſty's preſence, its qualities and peculiarities.

of which ſamples may be ſeen.

Samples of this cloth may now be ſeen in Mr. Maitland's warehouſe in Baſinghall-ſtreet; and it will be found, in converſing with Mr. Maitland and his partners, that in their opinion the R.'s of his Majeſty's wool, conſidered as a pile, are inferior to but few of the beſt of thoſe imported from Spain, though it is probable that no pile in Spain throws out ſo ſmall a proportion of F.s and T.s. From this opinion it may fairly be deduced, that his Majeſty's wool has improved ſince the ſheep were imported from Spain; indeed there is every reaſon to believe that it is ſtill improving, and will in a very few years equal if not excel the very beſt piles that have hitherto been imported into this kingdom.

The wool has improved.

Improvement of the carcaſe of Merino ſheep, by Mr. Tollet, without damaging the wool.

Mr. Tollet, a gentleman of Glouceſterſhire, who has purchaſed Merino ſheep both from the King and from Lord Somerville, has been very ſucceſsful in improving the carcaſe without damaging the wool; he poſſeſſes a ram, bred from a ram and a ewe both purchaſed from the royal flock in 1801; which, when clipped in June laſt, yielded 11 lbs. 12 oz. of unwaſhed wool. The carcaſe of this ſheep was then eſtimated by good judges at 16 lbs. a quarter, and it was admitted to be a handſome ſheep.

For this animal Mr. Tollet has refuſed an offer of 200 guineas, or of 100 for the next ſeaſon's uſe of him; he alſo refuſed 30 guineas each for the ſire and the dam, though old and infirm, being unwilling to part with animals which had belonged to the royal flock; he however ſold their ram lamb of the laſt year for 30 guineas, and thus made ſome progreſs in aſcertaining the value of this important breed.

Confirmation by the improvement of his Majeſty's flock.

Theſe facts, which prove an amelioration in the King's Merino ſheep, are fully confirmed by the improved ſhape and weight of his Majeſty's ſheerling rams of the preſent year, and give a juſtifiable hope, that by a due ſelection of rams and ewes, and a correct judgment in matching them, Merino ſheep will in time be produced, with carcaſes perfectly faſhionable, and wool as perfectly fine.

Lamb's wool,

No purchaſer having been laſt year found for the lambs wool at a price adequate to its value, it was made into light ladies cloth, which proves excellent, and promiſes to be a valuable article. A ſpeculation, however, has offered for manufacturing the lambs wool into ſuperfine woolen hoſe, which ſeems likely to yield a ſtill better price for the raw article than the cloth.

The demand from the growers proves that theſe facts are well eſtabliſhed;

The demand for his Majeſty's Merino ſheep increaſes at preſent beyond all calculation. The beſt informed clothiers in Glouceſterſhire, enlightened no doubt by the uſeful labours of the Bath Society, and the valuable experiment of Dr. Parry, as well as by the Doctor's, and by Lord Somerville's publications, are among the moſt anxious applicants to purchaſe. The Bath Agricultural Society, whoſe attention has been moſt particularly directed to the improvement of Engliſh wool, humbly requeſted the King to give them a Spaniſh ram, which requeſt his Majeſty moſt graciouſly complied with laſt Autumn, and they returned thanks in the warmeſt terms of reſpectful gratitude and ſatisfaction.

and render it juſt that the ſheep ſhould be diſtributed by the competition of a public ſale.

As ſpeculation on the value of Spaniſh ſheep is evidently on the increaſe, and a reaſonable probability now appears that his Majeſty's patriotic exertions in introducing the breed, will at laſt be duly appreciated and properly underſtood, it would be palpably unjuſt ſhould the views of thoſe who wiſh to derive a fair advantage from the ſale of the progeny of Spaniſh ſheep purchaſed by them from the royal flock, be in future impeded by a continuation of the ſale of the King's ſheep at prices below their real value.

This circumſtance having been ſtated to the King, his Majeſty was graciouſly pleaſed to permit the rams and ewes that are to be parted with from the royal Merino flock this year, to be ſold by auction, in the ſame manner as is done at Woburn by his grace the Duke of Bedford, and at Holkham by Mr. Coke, on the preſumption of this being the moſt likely manner of placing the beſt individuals of their improved breeds in the hands of perſons moſt likely to preſerve, and further to improve them.

JOSEPH BANKS.

Auguſt 17, 1803.

POSTSCRIPT.

Poſtſcript.

AS the publication of this report has been delayed by unavoidable circumſtances to ſo late a period, it is proper to add, that the wools of 1803 have yielded, both raw and ſcowered, much as uſual. The prime or R. of the ewe flock were ſold for 6*s.* 9*d.* a pound, and that of the Rams for 6*s.* 6*d.* Theſe enormous prices, however, depended on a ſcarcity of imported Spaniſh wool, and are highly diſtreſſing to the manufacturer: they ought not, therefore, to be allowed to enter into the ſpeculation of the grower.

Notice of ſale of the ſheep that can be ſpared.

The ſheep that can be ſpared from the royal flock will be ſold by auction this year at a barn oppoſite the Pagoda in Kew-lane, on the 15th of Auguſt next. Notice of the particulars will be given as ſoon as poſſible.

July 10, 1804.

X.

On the Oxides of Lead. By Thomas Thomson, *M. D.*

Communicated by the Author.

Great importance of the analyſis of metallic oxides:

PERHAPS there is no practical branch of chemical inveſtigation of more importance than the analyſis of the metallic oxides. Almoſt every thing relating to the metalline paints and ſalts depends upon it; and it involves, either directly or indirectly, moſt of the intereſting queſtions in the theoretical department of chemiſtry.

firſt applied with effect by Bergman and Scheele.

Bergman and Scheele were, I believe, the firſt perſons who applied chemical analyſis, in the modern ſenſe of the phraſe, to the metallic oxides. Notwithſtanding the difficulties with which theſe illuſtrious chemiſts had to ſtruggle, their experiments were made with ſo much care and ſagacity, that they ſtill furniſh us with the beſt data for aſcertaining the compoſition of ſeveral of theſe bodies. No modern chemiſt has laboured

Analyſes of Prouſt.

boured ſo ſucceſsfully in this department as Mr. Prouſt. To him we are indebted for the analyſis of the oxides of zinc, iron, tin, copper, antimony, and arſenic. His diſſertations are all ſtamped with the character of originality, and diſplay ſo much ſkill and candour, that they never fail to command the confidence of the reader. If he ſometimes puſhes his conſequences a little too far, he more than compenſates for this by the originality of his views, and the new light which he throws upon every ſubject that he diſcuſſes.

Subject of this memoir: the oxides of lead.

I intend at preſent to offer ſome obſervations on the oxides of lead, a ſubject more than once ſlightly touched upon by Prouſt, but never fully diſcuſſed by him. I truſt the difficulty of the ſubject will plead my excuſe, if I ſhall be unfortunate enough to fall into miſtakes.

Three diſtinct oxides; yellow, red, brown; and two others.

We are acquainted with three oxides of *lead* ſufficiently diſtinct from each other. The firſt is of a yellow colour, and forms the baſe of almoſt all the ſalts of lead; the ſecond is a paint well known by the name of *red lead;* the third a brown powder diſcovered by Scheele, and examined more lately by Prouſt and Vauquelin. Beſides theſe three, a fourth has been announced by Prouſt; and *litharge* has been conſidered by ſome as conſtituting a fifth. Let us examine theſe oxides.

I. *Yellow Oxide.*

Yellow oxide of lead.

The yellow pigment called *maſſicot* conſiſts eſſentially of this oxide; but the eaſieſt method of forming it, is to diſſolve lead in nitric acid. Pure lead diſſolves completely in that acid; but the lead of commerce uſually leaves a ſmall quantity of grey powder, which conſiſts for the moſt part of oxide of antimony, ſometimes mixed with a little ſilica. When the ſolution is concentrated by evaporation, we obtain cryſtals of nitrate of lead, a ſalt too well known to require any particular deſcription.

Nitrate of lead, loſes acid by low heat:

1. When the cryſtals of nitrate of lead thus obtained by evaporation, and well dried upon blotting-paper, are expoſed to a temperature of about 300°, they loſe, at an average, three *per cent.* of their weight. This loſs is not to be aſcribed to the eſcape of mere water, for the fumes ſmell ſtrongly of nitric acid.

— lead contained therein.

2. When 69 grains of lead are diſſolved in nitric acid, and the ſolution evaporated to dryneſs, the nitrate of lead, after

being dried at the temperature of 300°, weighs 112 grains. Hence 100 grains of lead yield $162\frac{2}{3}$ grains of nitrate of lead. From this we learn that 100 parts of nitrate of lead conſiſt of

$61\frac{2}{3}$ lead,
$38\frac{1}{3}$ foreign bodies.

100.

Carbonate of lead: white lead.

3. When 112 grains of nitrate of lead (dried at 300°) are diſſolved in water and mixed with a ſolution of carbonate of potaſh, a copious white powder precipitates, which is a *carbonate of lead*. Bergman ſhewed long ago, that the white lead of commerce is preciſely the ſame with this carbonate. When waſhed, collected on a filter, and dried at 300°, it weighs 90 grains. This ſhews us that 69 grains of lead yield 90 grains of carbonate: of courſe, 100 grains of lead would yield $130\frac{1}{2}$ grains of carbonate. From this experiment we learn, that 100 parts of precipitated carbonate of lead are compoſed of

$76\frac{2}{3}$ lead,
$23\frac{1}{3}$ foreign bodies.

100.

Carbonate of lead by heat leaves yellow oxide; 9 lead + 1 oxigen:

4. When 90 grains of precipitated carbonate of lead are expoſed in a retort to a heat gradually raiſed to redneſs, the acid and water which they contain are driven off, and a yellow coloured oxide remains behind. This oxide weighs 77 grains, and contains, of courſe, 69 grains of lead. Hence it follows, that the yellow oxide of lead is compoſed of 69 lead + 8 oxigen, or *per cent.* of

89.7 lead,
10.3 oxigen.

100.

— very fuſible.

It is well known that the oxides of lead very eaſily melt and run into glaſs. This happens in the preceding experiment, unleſs particular care be taken. In that caſe the lead acts with great energy upon the retort; but the loſs of weight is the ſame, unleſs the heat has been a great deal too high. When the oxide is fuſed in an earthen veſſel, it covers the ſurface with a yellow glaſs, as in the coarſeſt kinds of pottery. In that caſe ſome of the oxide may be diſſipated, unleſs the proper

Singular fact.

per precautions are taken. It deſerves attention, that when carbonate of lead is ſlowly heated in a glaſs retort till it begins to melt, the melted portion has a fine yellow colour, while the colour of what remains in the ſtate of a powder is a dirty pale brick-red; whereas in a platinum crucible the melted portion is red and the unmelted yellow.

Corrected elements of the carbonate.

5. From the preceding experiments it follows, that the yellow oxide of lead contains 10.3 *per cent.* of oxigen. Mr. Prouſt has deduced nine *per cent.* as the proportion of oxigen, from his experiments. This reſult does not differ much from mine. If I have committed an error, the oxigen I think is rated too high; for the lead which I uſed contained 1½ *per cent.* of antimony, the oxides of which have much more oxigen than the yellow oxide of lead. Perhaps we ſhall come nearer the truth by taking the mean of the two reſults: we may therefore conſider the yellow oxide of lead as compoſed of

90½ lead,
9½ oxigen.

100.

Comp. parts nitrate of lead, &c.

6. The preceding experiments enable us to ſtate the conſtituents of nitrate of lead as follows:

1. *Nitrate dried on Blotting-Paper.*	2. *Nitrate dried at 300°.*
66 yellow oxide,	68.5 yellow oxide,
34 acid and water.	31.5 acid and water.
100	100.0

They give us alſo the precipitated carbonate of lead, dried at 300°, as follows:

86 yellow oxide,
14 acid and water.

100.

The native carbonate of lead contains about 16 *per cent.* of carbonic acid. Precipitated carbonate then either contains leſs acid than native, or it loſes a part at a low heat. It is well known that carbonates, when in cryſtals, frequently contain more acid than when in the ſtate of powders.

Characters and habitudes of the yellow oxide.

7. Yellow oxide of lead is a powder of a lively yellow colour, taſteleſs, inſoluble in water, but ſoluble in fixed alkalies and

acids. The alkaline ſolutions have a yellow tinge; but the acids are moſt frequently colourleſs. It readily melts when heated, and forms a yellow, ſemitranſparent, brittle, hard glaſs. It does not loſe oxigen gas when heated. In violent heats a portion of the oxide is diſſipated. When kept heated in the open air, its ſurface becomes brick-red. When mixed with metallic lead it runs, according to Prouſt, into a green glaze.

Yellow oxide by mere heat from the nitrate.

8. The yellow oxide may be obtained directly from the nitrate of lead, by expoſing that ſalt to a ſufficient heat; but the loſs of weight ſuſtained is uſually greater than it ought to be. I ſuſpect that this is one reaſon why Prouſt found ſo ſmall a proportion of oxigen in yellow oxide. One hundred grains of nitrate of lead (obtained by evaporation) were put into a ſmall Wedgewood crucible furniſhed with a lid, and encloſed in a common earthen-ware crucible. They were expoſed for half an hour to an intenſe red heat in a wind-furnace. The ſalt was converted into a very hard, yellow, brittle glaſs, nearly opake: It had ſuſtained a loſs of 40 *per cent.* or about ſix *per cent.* more than it ought to have loſt. On breaking this glaſs to pieces the reaſon of this became obvious: It contained a great number of globules of lead reduced to the metallic ſtate, ſome of them of conſiderable ſize. From this experiment we learn, that lead is reducible directly from the nitrate merely by the application of heat, without adding any combuſtible matter.

II. *Suppoſed firſt Oxide.*

Aſhes of lead, or ſuppoſed firſt oxide. It is yellow oxide and metallic lead.

When lead is kept melted in the open air, it is ſoon covered with a dirty coloured powder, formerly called the *aſhes* of lead. When this powder is heated ſufficiently, it melts into a greeniſh yellow glaſs, in which globules of lead may be detected. Mr. Prouſt has ſhewn, that theſe aſhes are a mixture of the yellow oxide of lead with lead in the metallic ſtate. They do not, therefore, conſtitute a peculiar oxide.

Neither is the *white oxide* of the French chemiſts entitled to a place among the oxides of lead; being in all caſes nothing more than the yellow oxide combined with an acid, uſually the carbonic.

Prouſt's oxide by boiling lead in the nitrate.

But Mr. Prouſt, in his obſervations on the *Connoiſſances Chimiques* of Fourcroy, has mentioned the method of forming an oxide of lead containing leſs oxigen than the yellow. When lead is boiled in a ſolution of nitrate of lead, the liquid gradually aſſumes a yellow colour, and, on cooling, depoſits cryſtals in ſcales. Theſe cryſtals, according to Prouſt, contain the oxide in queſtion: But his concluſions, as far as appears, were formed from the ſingle experiment related. He does not ſeem to have decompoſed the ſalt, nor to have examined its baſe.

The experiment.

1. When 100 grains of nitrate of lead are diſſolved in water, and boiled in a phial with a cylinder of lead (weighing 64½ grains), the metal ſoon loſes its brilliancy and is covered with a white cruſt, while the liquid aſſumes a yellow colour. The boiling was continued (water being added as faſt as it evaporated) till the liquid ſeemed to exert no farther action on the lead. The cylinder being then taken out and weighed, was found to have loſt 44 grains. From this we learn, that 100 grains of nitrate of lead diſſolved in water, are capable of uniting with 44 grains of lead, or almoſt half their weight. The whole, however, was not diſſolved. A bluiſh-grey powder fell to the bottom, and increaſed in quantity as the cylinder diminiſhed. If theſe 44 grains were oxidized at the expence of the yellow oxide of the nitrate, we ſhould have a new oxide containing much leſs oxigen; and it would be eaſy to aſſign the proportion of its conſtituents; for 100 grains of nitrate contain 66 grains of yellow oxide, compoſed of 59¾ lead and 6¼ oxigen: Therefore the new oxide contains 59¾ + 44 lead and 6¼ oxigen, or, *per cent.*

94·3 lead,
5.7 oxigen.
———
100.0

But it is extremely unlikely that the 44 grains of lead ſhould receive the whole of the oxigen neceſſary to enable them to diſſolve from the oxide, while an exceſs of nitric acid is preſent in the ſolution. Let us therefore examine the new ſalt.

Depoſition of ſcaly cryſtals by cooling, and ſmall needles by evap. White depoſ. of carbonate by cold water.

2. When the ſolution cools, it depoſits thin ſcaly cryſtals of a light yellow colour: They have the ſame ſweet aſtringent taſte as common nitrate, but are leſs ſoluble in water. If the yellow liquid which remains be farther concentrated, it depoſits, on cooling, ſmall needles of a pale yellow colour, not unlike *ſugar of lead*. Their taſte is ſweet and aſtringent; they are not altered by expoſure to the air. When thrown into cold water they fall to the bottom, the liquid gradually be-

comes milky, and depoſits a white powder. This powder does not diſappear, though the ſolution be heated boiling hot; but the liquid acquires the property of diſſolving an additional quantity of the ſalt, without depoſiting any more white powder. Boiling water diſſolves the ſalt without any ſimilar depoſition. Hence I think we may conclude, that the white powder is owing to the preſence of ſome carbonic acid in the cold water, and that our ſalt in this reſpect reſembles acetate of lead.

The ſalt depoſited in the boiling, contained yellow oxide and an under proportion of acid;

3. When 30 grains of the ſalt depoſited during the boiling of the lead in the nitrate, were cautiouſly heated to redneſs, they melted into a yellow maſs, which weighed 24.5 grains. The loſs of 5.5 grains muſt be aſcribed to acid and water. Hence this ſalt is compoſed of

81.5 oxide,
18.5 acid and water.

100.0

Theſe 24.5 grains of oxide being diſſolved in nitric acid, yielded 35 grains of common nitrate of lead (dried at 300°). But 35 grains of nitrate contain 24 grains of yellow oxide, which ſcarcely differs from the quantity diſſolved. From this experiment it ſeems to follow, that the ſalt in queſtion contains only yellow oxide, and that it differs from common nitrate in containing a ſmaller proportion of acid. But it will be ſaid, perhaps, that the oxide of the ſalt abſorbed oxigen from the nitric acid during the application of the heat, and was thereby oxidized up to the ſtate of yellow oxide.

as alſo the cryſtals do.

4. Twenty-three grains of the needle-form cryſtals were diſſolved in water and decompoſed by carbonate of potaſh. The carbonate had the common appearance, and, when dried in 300°, weighed 24 grains. But 24 grains of common carbonate contain about 21½ of oxide, and ought therefore, when diſſolved in nitric acid, to yield about 31½ grains of common nitrate of lead (dried at 300°); and, upon trial, I found this to be the caſe very nearly. The oxide in the needle-form cryſtals then is the yellow; for there is no apparent courſe from which, in the above experiment, oxigen could be drawn. And if this be the caſe with the needles, it muſt be ſo alſo with the ſcaly cryſtals; for the two ſalts are obviouſly the ſame.

5. Sixty-

The oxide of Prouſt's ſalt is not different from the yellow.

5. Sixty-three grains of the ſalt, partly in ſcales and partly in needles, were fuſed with carbonate of potaſh in a Wedgewood crucible. By ſolution and filtration a fleſh-coloured powder was obtained, which was a mixture of oxide of lead and ſilica. It weighed 53 grains; but a portion which I could not eſtimate adhered to the crucible. The ſilica was obviouſly abraded from that veſſel. Thirty grains of this powder digeſted in nitric acid, left 3½ grains of ſilica; of courſe, 26½ were diſſolved. The ſolution yielded 39½ grains of nitrate of lead. Now 39½ of nitrate contain 27 grains of yellow oxide, or almoſt the very quantity diſſolved. The oxide obtained by this experiment, then, was the yellow; of courſe, it coincides exactly with the preceding ores.

Prouſt's ſalt, then, does not appear to contain a different oxide from common nitrate; but its new properties were owing to the different proportion of its acid. It is completely neutralized, whereas common nitrate contains an exceſs of acid, and is, in fact, a ſuper-nitrate. But if this concluſion be well founded, Prouſt's nitrate may be formed by expoſing common nitrate to a heat ſufficient to expel the exceſs of acid. It was requiſite to verify this preſumption by experiment.

Prouſt's ſalt made by abſtracting acid inſtead of adding lead.

6. One hundred grains of nitrate of lead (dried in 300°) were expoſed to a graduated heat in a flaſk. Fumes of nitrous acid ſeparated in abundance, and the ſalt loſt five *per cent.* of its weight. On increaſing the temperature the ſalt melted into a tranſparent glaſs of a very pale yellow colour. The weight of the maſs was now reduced to 85 grains. Hence it was compoſed of 68.5 oxide and 16.5 acid, or, *per cent.* of

80 oxide,
20 acid.
———
100.

On pouring water into the flaſk and digeſting, I obtained a yellow ſolution ſimilar to that formed by boiling lead in nitrate of lead, but not ſo deep. A yellow powder refuſed to diſſolve; it conſiſted chiefly of the portion of ſalt at the bottom of the flaſk, which had been expoſed to a higher temperature. It was taſteleſs, and not unlike ſub-muriate of lead. When heated to redneſs it melted into a yellow glaſs, and loſt 14 *per cent.* It was therefore compoſed of

86 oxide,
14 acid and water.
———
100.

The ſolution being evaporated, depoſited two ſets of cryſtals; one ſet conſiſting of common nitrate of lead, another ſet reſembling thoſe obtained by Prouſt.

Three nitrates of lead; *viz.* neutral, and with exceſs or with defect of acid.

7. From the preceding details we learn, that there are three diſtinct ſpecies of nitrated lead: The firſt is a *ſuper-nitrate*, or contains an exceſs of acid; the ſecond is neutral; the third contains an exceſs of baſe, and is, of courſe, a ſub-nitrate. The firſt ſpecies includes the common nitrate of chemiſts in all its varieties; the ſecond, the nitrate of Prouſt; the third, the yellow powder obtained by heating common nitrate ſufficiently.

III. *Broun Oxide.*

Brown oxide; left when minium is diſſolved in nitric acid, &c.

Though this oxide contains a maximum of oxigen, I beg leave to introduce it here, becauſe the knowledge of its compoſition is neceſſary to enable us to analyſe the *red oxide* of lead. It was diſcovered by Scheele, and deſcribed by him in his diſſertation on manganeſe. When diluted nitric acid is poured upon red lead, the greater part of the oxide is diſſolved, but a brown powder remains behind, which is not acted upon by the acid. This brown powder is the brown oxide of lead. Prouſt diſcovered that it may be formed alſo by cauſing a current of oxi-muriatic acid gas to paſs through red lead ſuſpended in water.

Its habitudes;

1. This oxide is a taſteleſs powder, of a flea-brown colour, and very fine and light. It is not acted on by ſulphuric nor nitric acid. To muriatic it gives out oxigen, and converts it into oxi-muriatic acid. Oxi-muriatic acid diſſolves it, and forms two ſalts, muriate and hyper-oxi-muriate of lead. The vegetable acids reduce it to the ſtate of yellow oxide. Fourcroy, on the authority of Vauquelin, affirms, that ſulphur takes fire when triturated with brown oxide of lead. With me the experiment did not ſucceed: I ſuſpect, therefore, that the oxide uſed by Vauquelin contained a portion of hyper-oxi-muriate of lead mixed with it.

contains one-tenth more oxigen than yellow

2. When 100 grains of this oxide, prepared from red lead by nitric acid, are expoſed to a red heat, they loſe nine grains

of their weight, and are converted into yellow oxide: Theſe nine grains are oxigen gas. Hence brown oxide is compoſed of 91 yellow oxide and 9 oxigen. But 91 of yellow oxide contain 9.4 of oxigen.* Therefore 100 parts of brown oxide are compoſed of

oxide; eaſily diſengaged.

81.6 lead,
18.4 oxigen.

100.

3. Mr. Prouſt, from his experiments, ſtates the proportion of oxigen in this oxide at 21 *per cent.* If we take the mean of the two reſults, we obtain 19.7. We may, therefore, lay down 20 *per cent.* as the proportion of oxigen in brown oxide of lead: This cannot deviate far from the truth.

IV. *Red Oxide.*

Red lead;

Red lead being one of the moſt common of pigment, is unknown, I preſume, to no perſon. The method of manufacturing it has been deſcribed by Dr. Watſon in his *Chemical Eſſays*, by Jars in the *Memoires* of the French Academy for 1770, and by Ferber in his Mineralogy of Derbyſhire.

its characters;

1. It is a taſteleſs powder, very heavy, and of an intenſe red colour, often inclining to orange. I have never met with any ſpecimens of it abſolutely pure, but not unfrequently the foreign bodies do not exceed one or two *per cent.* They conſiſt of ſeven grains of ſand and oxide of antimony. Dr. Watſon found traces of ſilver in it. It loſes no ſenſible weight in a heat of 400°.

contains 88 lead + 12 oxigen;

2. When 50 grains of red lead are digeſted in diluted nitric acid, they leave 12 grains of brown oxide. The ſolution evaporated to dryneſs, yields 56 grains of nitrate of lead. Now, 56 grains of nitrate contain 38.36 grains of yellow oxide. Red lead, therefore, is compoſed of 38.36 yellow oxide and 12 brown oxide, or, *per cent.* of

76.72 yellow oxide,
24.00 brown oxide.

100 72

The exceſs muſt be aſcribed to the imperfection of our methods. I ſhall omit it in the calculation: Not that red lead is a mixture of yellow and brown oxides, but that it contains all the lead and oxigen in the above proportions of theſe bodies. Now

76 grains yellow oxide is compoſed of 68.8 + 7.2
24 grains brown oxide of - - - 19.2 + 4.8

Therefore red lead is compoſed of - 88. + 12. = 100.

not eaſily decompoſed by mere heat.

3. It is well known that red lead gives out oxigen gas when heated, and that it approaches to the ſtate of yellow oxide. The loſs of weight ought to give us the portion of oxigen which it contains more than is neceſſary to conſtitute it yellow oxide: But, upon trial, I could obtain no ſatisfactory reſults this way. In one experiment 100 grains of red lead loſt 4½ *per cent.* in another ſeven *per cent.* The experiments were made in ſmall covered earthen-ware crucibles. The oxide was melted into a dark-brown tranſparent glaſs, not unlike glaſs of antimony, but much harder. On breaking this glaſs, I found in it globules of lead reduced to the metallic ſtate: This accounts in part for the loſs of weight, and ſhews us alſo, contrary to the opinion of chemiſts, that the red oxide of lead is reducible, at leaſt in part, by mere heat. In all probability nothing prevents the complete reduction but the readineſs with which it unites with the veſſel in which the experiment is made. I twice varied the experiment, by encloſing the ſmall earthen crucibles containing the red lead in a crucible of platinum; but in neither caſe did I obtain any viſible metallic globules; yet the loſs of weight was the ſame. This renders it probable that a portion of lead had been reduced and afterwards diffuſed through the oxide.

Not ſoluble *as red lead* in acids, nor in alkalis.

4. The red oxide of lead does not ſeem capable of combining with acids. Many acids indeed act upon it, but they always begin by reducing it to the ſtate of yellow oxide. The fixed alkalies do not alter its colour, but they gradually diſſolve it. From this ſolution it is thrown down always in the ſtate of yellow oxide: Hence it muſt loſe oxigen during the ſolution.

V. *Litharge.*

Though litharge is very far from being the ſame with red lead, yet, as the mode of preparation is analogous, a few remarks on it may not be improper in this place.

Litharge;

Litharge conſiſts of ſcales, partly of a golden yellow colour and partly red: They poſſeſs a certain degree of elaſticity. The method of making *litharge* has been deſcribed by Dr. Watſon, by Gmelin, and by other chemical writers.

contains carbonate,

1. When 100 grains of litharge are expoſed to a red heat, they melt into a yellow glaze, and loſe, at an average, four grains of their weight. When 50 grains of pounded litharge are thrown into nitric acid, they diſſolve with efferveſcence, and loſe two grains of their weight. The efferveſcence and loſs of weight are owing to the eſcape of carbonic acid gas. From this we may conclude, that litharge contains four *per cent.* of carbonic acid.

and antimony:

2. When 50 grains of litharge were diſſolved in nitric acid, and the ſolution evaporated to dryneſs and re-diſſolved in water, $1\frac{1}{2}$ grains of a grey powder remained behind in my trials, which proved to be oxide of antimony. Therefore, litharge contains three *per cent.* of oxide of antimony.

Comp. parts inveſtigated:

3. The ſolution evaporated to dryneſs, gave 68.5 grains of nitrate of lead; but this nitrate contains 46.72 grains of yellow oxide of lead. Of courſe, we have litharge compoſed of

93.44	yellow oxide of lead,
3.00	oxide of antimony,
4.00	carbonic acid.
100.44	

The ſmall exceſs muſt be aſcribed to unavoidable errors in the analyſis.

4. Fifty grains of litharge diſſolved in nitric acid, deprived of its oxide of antimony, and then thrown down by carbonate of potaſh, gave $52\frac{1}{2}$ grains of carbonate of lead. Hence 97 grains of litharge (ſuppoſing the antimony a foreign body) would have given 105 grains of carbonate. But 97 grains of litharge contain nearly four of carbonic acid. Hence we have the carbonate formed of 93 oxide and 12 acid.

In this experiment the carbonate produced was too ſmall by about a grain. This was partly owing to the loſs of a ſmall quantity of the powder while ſeparating it from the filtre. As I could not eſtimate the loſs, I left it out in the calculation, and ſtated the amount preciſely as I found it.

it is a ſubcarbonate:

From the preceding experiments it follows, that litharge is a ſub-carbonate of lead, ſince it conſiſts eſſentially of about 96

96 yellow oxide,
4 carbonic acid.
——
100.

Probably it varies ſomewhat in the proportion of its conſtituents, according to circumſtances: But all my trials were made on one parcel of litharge. I have obſerved traces of carbonic acid alſo occaſionally in red lead, but too little to affect its weight.

VI. *Concluſions.*

Summary recapitulation.

From the preceding experiments and obſervations we are entitled, I think, to draw the following concluſions:

1. Three oxides of lead only are at preſent known.[*] The conſtituents of theſe oxides may be ſeen in the following table:

Oxides.	*Colour.*	*Conſtituents.*	
		Lead.	Oxigen.
Protoxide.	Yellow.	90.5	9.5
Deutoxide.	Red.	88	12
Peroxide.	Brown.	80	20

Lead. *Oxigen.*
100 + 10.6 = 110.6 protoxide.
100 + 13.6 = 113.6 deutoxide.
100 + 25. = 125. peroxide.

2. The aſhes of lead are a mixture of protoxide and lead in powder.

3. White lead and litharge are combinations of protoxide with carbonic acid: the firſt is a carbonate, the ſecond a ſubcarbonate of lead.

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[*] As colour is a very ambiguous criterion for diſtinguiſhing metallic oxides, I have been accuſtomed for ſome time to denote the oxide with a minimum of oxigen by prefixing the Greek ordinal number to the term oxide: Thus, *protoxide of lead* is lead united to a minimum of oxigen. The oxide with a maximum of oxigen I call *peroxide*: Thus, *brown oxide of lead* is the *peroxide* of lead. I denominate the intermediate degrees of oxidizement by prefixing the Greek ordinals 2d, 3d, 4th, &c. Thus, *deutoxide* is the ſecond oxide of lead, *tritoxide* of cobalt, the third oxide of cobalt, and ſo on.

4. The yellow nitrate of Prouſt contains the ſame oxide as common nitrate. But in it there is no exceſs of acid; whereas common nitrate is in fact a *ſupernitrate* of lead. In a ſtrong heat it is partly converted into a *ſubnitrate*.

5. Protoxide of lead unites with all acids, deutoxide with none, and peroxide only with hyperoxymuriatic acid.

6. The protoxide of lead may be formed by combuſtion; but the other two cannot, and indeed loſe oxygen in a ſtrong heat. The deutoxide is formed by keeping protoxide in contact of air at a given temperature: the peroxide by the action of nitric or oxymuriatic acid on the deutoxide.

XI.

On certain Chemical Effects of Light. In a Letter from WM. HYDE WOLLASTON, *M.D. F. R. S.*

To Mr. NICHOLSON.

SIR,

Introduction on the inviſible rays that have chemical effects.

HAD I foreſeen the publication of Mr. Ritter's '*Experiments on Light*' in the laſt number of your Journal,* I would have requeſted you to accompany them with a few obſervations of mine on the ſame ſubject; not with a view of claiming any priority in the obſervation of thoſe inviſible rays, that have chemical effects, which I believe occurred to Mr. Ritter and myſelf very nearly at the ſame time; but for the purpoſe of inſerting a caution againſt the theory implied by the term "*diſoxidating*" as applied to thoſe rays.

The power of theſe rays was termed *chemical* by Dr. W. and not *diſoxidating*, becauſe they do not conſtantly diſoxidate.

In my note upon a communication to the Royal Society,† which you did me the honour to reprint in the 4th Vol. of your Journal,‡ I was careful to expreſs the power exerted by the moſt refrangible rays on muriate of ſilver, in general terms as *chemical*, not merely from a doubt whether they would in other caſes produce a correſponding effect, but becauſe I had at that time made the following experiments, which proved that the ſame rays, which cauſe the emiſſion of oxygen by muriate of ſilver, occaſion its abſorption by the reſin uſually called gum guaiacum.

* Page 214. † Phil. Tranſ. 1802, p. 379. ‡ 8vo. ſeries. p. 99.

My reaſons for withholding theſe experiments at that time were, that they appeared ſomewhat irrelevant to the primary ſubject under diſcuſſion, and that I was alſo in hopes of increaſing their value by additional trials on other ſubſtances.

Whether the ſolar rays tend to extinguiſh fire; —not proved.

Upon conſidering the power which theſe rays poſſeſs of expelling oxygen from the muriate of ſilver, I thought it not impoſſible, that there might be more truth than I had been accuſtomed to ſuppoſe, in the popular obſervation that the ſun has a tendency to extinguiſh fire, as the ſame rays might retard combuſtion by oppoſing the abſorption of oxygen. Accordingly I made various experiments on different ſubſtances in a ſtate of ſlow combuſtion, but without any apparent confirmation of that hypotheſis.

Light or ſolar rays did not affect vegetable blues;—

I alſo tried the action of light on ſeveral vegetable blue colours, which are known to be affected by union with oxygen, and upon the ſame colours previouſly reddened; but on theſe alſo I did not ſucceed in producing any effect at either boundary of the priſmatic ſpectrum.

Trial of guaiacum.

After failure of theſe endeavours, I had recourſe to guaiacum, which I had long known to acquire a green colour by expoſure to light; but that the preſence of air is alſo requiſite for this purpoſe I had aſcertained in the following manner.

No change by ſolar rays without air.

Two plates of glaſs were heated with a ſmall piece of guaiacum interpoſed, and thereby cemented together in their centers for a circular ſpace about 1½ inch diameter. In this ſtate they were expoſed for ſeveral weeks during ſummer to the ſun, without the ſmalleſt apparent alteration in the colour of the guaiacum.

—but when the air had acceſs;

The plates were then forcibly ſeparated; and as they were both ſimilarly coated with a portion of the reſin adhering to their ſurfaces, one of them was preſerved for compariſon in a dark place, where it had free acceſs of air alone, while the other was again expoſed uncovered to the mid-day ſun.

—it was affected in five minutes by the noon-day ſun.

The latter was in five minutes perceptibly rendered green, and in a few hours had acquired the full colour, which it ſeemed capable of receiving; but the former, in the courſe of many months that it was kept confined from the light, ſeemed not to have been diſcernibly altered.

The priſmatic ſpectrum was too weak to ſhow whether all the rays were effective or the contrary.

Since by later experiments it appeared probable, that the whole of the ſun's rays were not active in this proceſs, with a view to determine on what part of them the effect might depend,

pend, I diſſolved ſome guaiacum in alcohol, and after having waſhed a card with the tincture, I expoſed it for ſome time to different parts of the common priſmatic ſpectrum, but without producing any apparent change. It therefore became neceſſary to have recourſe to other expedients for increaſing the power of the ſpectrum.

Over the ſurface of a lens 7 inches in diameter, was paſted a circular piece of paper having its radius ⅒ of an inch leſs than that of the lens. I had conſequently remaining uncovered a priſmatic annulus, correſponding in the length of its circumference to a priſm 22 inches long, ſo arranged by its circular form that any one of the colours might at pleaſure be brought to a focus, or the ſpectrum might be received as a ring of any diameter required, by mere variation of the diſtance of the lens. At ſhort diſtances the exterior margin of the ſpectrum of courſe was red, and the violet within. The focus of brighteſt illumination was at 24¼; at greater diſtances the ſpectrum again became an annulus with its colours in an oppoſite order to the preceding, having the violet on the exterior margin.

A broad convex lens being covered all but a narrow exterior ring, gave, at pleaſure, according to the diſtance, a focal ſpot of any one colour, or an annular ſpectrum, &c.

With this apparatus the effect produced on muriate of ſilver is much accelerated. At diſtances ſhort of 22¼ inches a ring is produced; at 22½ a circular dark coloured ſpot; and at about 23 inches appears to be the focus of theſe rays, as the ſpot is then ſmalleſt; at 23½ it is larger, at 24¼ it again becomes a ring ſhaded to the center; and at 24½, (unleſs the paper has been wetted,) the center remains compleatly white though ſtrongly illuminated. I have not however been able in any ſituation to reſtore the white colour to muriate of ſilver, after it has once been tinged, however ſlightly, by expoſure to the moſt refrangible rays.

Muriate of ſilver very ſpeedily affected by theſe concentrated rays;—

but not reſtored to whiteneſs.

The experiments on guaiacum nevertheleſs will prove diſtinctly, that the powers of the two extremities of the ſpectrum are not only different, but oppoſite in their chemical effects.

Guaiacum was affected both ways.

A ſufficient quantity of paper having been tinged with the ſolution of guaiacum, was cut into ſmall pieces, ſome of which were expoſed to the ſun-ſhine till rendered compleatly green; the reſt were kept confined from the light till taken out for each experiment.

The firſt endeavour was to aſcertain the focal diſtance of thoſe rays which gave the deepeſt colour in a given time; and it was found to be about 23 inches diſtant from the lens. At

At a ſhort focal diſtance (namely of rays moſt refrangible) the deepeſt green was produced;—

ſhorter diſtances the ſurface coloured was larger, but in the ſame time paler. At 22¼ a green ring was formed, having its center without colour.

At a medium focus no effect was produced;

When the guaiacum was expoſed at diſtances greater than 23 or 23¼, the ſurface coloured was alſo larger, but much paler than at equal diſtances ſhort of the focus; inſomuch that at 24¼, which correſponds with the principal focus of illumination, little or no effect was produced in the ſpace of one minute, which was the time employed in other experiments. It was manifeſt therefore, that the chemical effect of the moſt refrangible rays (which were now diverging beyond their focus) was in this ſituation counteracted by an oppoſite action equally powerful, of the moſt refrangible (not yet arrived at their focus;) and as it appeared probable that the power, which could in one inſtance prevent diſcoloration, might alſo, when duly applied, remove the ſame colour after it had been produced. I next expoſed to the condenſed ſpectrum, at various diſtances from the lens, portions of the paper that had been previouſly rendered uniformly green. A ſecond focus was now found at the diſtance nearly of 25¼, in which the green colour was compleatly removed, and the guaiacum reſtored to its pale yellow colour.

At a longer focal diſtance (namely of rays leaſt refrangible) the original yellow was reſtored.

It is unneceſſary to deſcribe minutely the conſequences reſulting from variations of the diſtance, as the effects in this inſtance were neceſſarily the counterpart to the preceding; the circle of yellow was larger, when the paper was placed at a greater diſtance from the lens, and at the diſtance of 25⅞ the center remained green, ſurrounded by a yellow ring, correſponding nearly to the red and orange interior margin of the annular priſmatic ſpectrum.

In carbonic acid gas the diſoxidation, but not the oxidation was practicable.

The ſame experiments being afterwards repeated in carbonic acid gas, only confirmed the opinion before entertained, of the cauſes to which the changes of colour were owing, but afforded little additional information. In this gas the guaiacum could not be rendered green at any diſtance from the lens, but was ſpeedily reſtored from green to yellow by expoſure to the focus of red rays.

The removal of colour (or diſoxidation) was alſo effected by heat from a piece of metal.

Since the removal of colour was obſerved to take place in the ſituation of the principal focus of heat, it ſeemed deſirable to aſcertain whether the preſence of light, or the circumſtance of radiation had any influence in promoting this effect. A piece of paper was therefore ſtained with guaiacum and after being rendered green by expoſure for a ſufficient length of time to light, was preſſed on its poſterior ſurface with a ſilver ſpoon previouſly heated over the flame of a candle; and the green colour was thereby as effectually removed, as in the focus of ſolar heat.

Radiant ſolar heat penetrates tranſparent bodies; culinary radiation ſcarcely at all.

The laſt experiment may poſſibly appear to have been unneceſſary; but until it is explained why the heat, that accompanies the ſun's rays, penetrates the ſubſtance of tranſparent or ſemi-tranſparent bodies, while the radiant heat from a fire has ſcarcely power to enter even the moſt tranſparent, but principally ſcorches the ſurface, and is thence ſlowly conducted into the interior parts: no degree of caution upon a ſubject ſo imperfectly underſtood, ſhould be deemed ſuperfluous.

I remain, Sir, &c.
W. WOLLASTON.

XII.

Illuſtrations of Mr. Dalton's Theory of the Conſtitution of Mixed Gaſes. In a Letter from Mr. WM. HENRY, *of Mancheſter, to Mr. Dalton. Communicated by the Writer.*[*]

To Mr. DALTON.

DEAR SIR,

Cauſes why a new theory may not readily produce conviction and adoption.

IN the firſt enunciation of a new theory, it is not unuſual that ſome links are omitted in the chain of reaſoning, which led to its formation; and thus the doctrine fails of that ready and general acceptance, which immediately follows its more diſtinct development. Such an omiſſion appears to me to have taken place in your Theory of the Conſtitution of Mixed Gaſes; for, according to your own candid confeſſion, ſeveral perſons, verſed both in chemical and mechanical ſcience, have declared their inability fully to underſtand the ſcope of the hypotheſis, and conſequently to judge of its merits or defects. In the diſcuſſions alſo, which took place in this Society, on your ſeveral papers, the doctrine was oppoſed by almoſt every member intereſted on ſuch ſubjects, and by no one more ſtrenuouſly

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[*] Read before the Mancheſter Society.

nuouſly than myſelf. Subſequent attention, however, to the evidences of the theory, and ſtill more the reſults of experiments which were made under impreſſions very unfavourable to the hypotheſis, have ſatisfied me that the oppoſition to it aroſe chiefly from an imperfect comprehenſion of the argument; and that your theory is far better adapted than any former one, for explaining the relation of mixed gaſes to each other, and eſpecially the connection between gaſes and water.

Diſtinguiſhing principle of Dalton's theory.

The diſtinguiſhing principle of your doctrine I apprehend to be, that *mixed gaſes neither attract nor repel each other, and that every gas is as a vacuum to every other gas.* It is not my intention to recapitulate your proofs of this poſition, but merely to add to them the evidence of a few facts, which have occurred to me, and which ſtrongly tend to eſtabliſh the ſame concluſion.

Since gaſes are held in water (always) mechanically in quantities as the preſſure, different gaſes do not preſs each other if they do not prevent eſcape.

From a ſeries of experiments, which I communicated to the Royal Society, and which appeared in their Tranſactions for 1803, it may, I think, be ſafely inferred, that the relation of gaſes to water is altogether a mechanical one; for the quantity abſorbed follows exactly the ratio of the preſſure. If then it can be ſhewn that a gas, abſorbed by water, is not retained in its place by an atmoſphere of any other gas, we ſhall be furniſhed with a ſtrong preſumption that different gaſes do not gravitate on each other.

Carbonic acid quits water expoſed to the atmoſphere,

It is well known that water may be charged with its own bulk, or rather more, of carbonic acid gas, under a preſſure of 30 inches of mercury. The gas, thus abſorbed, is retained ſo long as the water is preſerved from contact with any other gas; but, when expoſed to the atmoſphere, the carbonic acid gas rapidly eſcapes. Now this effect can be only aſcribed to one of two cauſes, 1ſt, the affinity of carbonic acid for atmoſpheric air may ſurpaſs that of its affinity for water; or, 2dly, the air of the atmoſphere does not preſs on the gas in the water, which is therefore placed under ſimilar circumſtances, as if expoſed under the exhauſted receiver of an air-pump.

—becauſe attracted, or elſe becauſe not preſſed by the atmoſphere.

Not from attraction; for atmoſpheres of different gas cauſe no difference.

Were the firſt explanation the true one, it might be expected that equal quantities of various gaſes would detach different quantities of carbonic acid from like volumes of impregnated water; becauſe the affinities of theſe gaſes, as in all other caſes of chemical affinity, differing in force, would occaſion their combining with different quantities of carbonic acid, and in a certain order. But on making the experiment, with all the attention I could beſtow, this did not prove to be the fact: for ſimilar meaſures of impregnated water gave up equal bulks of carbonic acid, to like quantities of all the different gaſes.

The reverſe of this fact alſo occurred to me in the courſe of a ſeries of experiments, to which I have already referred; viz. that the admixture of common air with carbonic acid gas diminiſhes conſiderably the proportion of the latter gas taken up by water. Thus, when 20 meaſures of pure carbonic acid gas are agitated with 10 of water, at leaſt 10 meaſures of gas are abſorbed. But from a mixture of 20 meaſures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid. That chemical affinity between the mixed gaſes is not the cauſe of the diminiſhed amount of abſorption, is perfectly clear; ſince it is indifferent, as to the effect, what gas is added, and the proportion alone influences the reſult. The effect is therefore to be aſcribed to the diminiſhed denſity of the ſuperincumbent carbonic acid by mixture with another gas; and the preſſure of gaſes being directly as their denſity, and the quantity abſorbed by water being as the preſſure, the abſorbed carbonic acid muſt neceſſarily quit the water. This eſcape continues till the carbonic acid *above* the water has a denſity equal to that *in* the water, and no longer.

If carbonic acid be mixed with another gas, the abſorption by water is governed by the denſity of the carbonic acid and not by the nature of the gas.

Previouſly to my acquieſcence in your theory of mixed gaſes, I undertook an extenſive ſeries of experiments, with a view to aſcertain the order of affinities of gaſes for water. But, after a great variety of trials, made with all the accuracy in my power, I could diſcover nothing like a ſeries of elective attractions. Each gas, it was found, diſplaced every other, and reciprocally was diſlodged by them.

Water has no preference of attraction as to the gaſes.

It may be urged againſt the doctrine of the non-gravitation of gaſes on each other, that from water impregnated with carbonic acid gas, and expoſed to the atmoſphere, the gas ought, on this principle, to eſcape as rapidly as under an exhauſted receiver. It muſt be remembered, however, that the eſcaping gas conſtitutes, by admixture with the air of the atmoſphere, a gas of diminiſhed denſity, but ſtill of ſuch denſity as to retard the eſcape of farther portions. All that the air-pump effects is to remove theſe as faſt as they are liberated.

The ſlower eſcape of a gas from water expoſed to the atmoſphere compared with a vacuum ariſes from the leſs rapid exhauſtion of the ſuperincumbent gas.

There are various facts, ſatisfactorily explained on this doctrine, which are irreconcilable to any former hypotheſis. Of

Facts explained by Dalton's theory.

theſe I ſhall mention only a few; ſince the theory will receive from yourſelf all the elucidation that its eſtabliſhment can require.

Light and heavy gaſes mix ſpontaneouſly.

1. If each gas be a vacuum to every other, a heavier gas ſhould aſcend into a lighter one, without the aid of agitation; and on the contrary a lighter one ſhould deſcend into a heavier one. That this is actually the fact, and under circumſtances very unfavourable to their mixture, your own experiments have fully proved.

Sulphuret takes oxigen from air not agitated.

2. The hypotheſis explains why ſulphuret of potaſh withdraws oxygen from the air without agitation, and whether placed at the top or at the bottom of a jar; for it acts as if the abſorbed gas were the only one preſent in the veſſel.

Abſorbable gaſes expel the laſt portions of common air from water.

3. It explains why the laſt portions of common air are expelled from water by carbonic acid, and other abſorbable gaſes. For theſe gaſes act as a vacuum to the air contained in the water, which muſt therefore neceſſarily quit its place. It ſolves alſo the problem how to expel completely any gas from water; for to effect this, the water muſt ſucceſſively be agitated with portions of ſome other gas of the greateſt attainable purity. Thus to expel atmoſpherical air entirely from water, it may be agitated with pure carbonic acid gas; but as the liberated common air preſſes on that remaining in the water, according to the proportion it bears to the ſuperincumbent carbonic acid, the gas thus employed muſt be removed, and freſh and pure portions uſed in ſucceſſion.

Beſt method of impregnating water with a gas.

4. By applying the ſame general law, we are taught how to effect the higheſt attainable impregnation of water with any gas. There could be no difficulty in accompliſhing this object, if the gas and water were both abſolutely uncontaminated by admixture with other gaſes; but when pure carbonic acid is agitated with water, atmoſpherical air is extricated, which, mingling with the carbonic acid, leſſens its denſity. To obviate this difficulty as much as poſſible, a quantity of water, to be impregnated fully with carbonic acid, ſhould be agitated with ſeveral ſucceſſive portions of the pureſt poſſible gas. The unabſorbed reſiduum ſhould alſo be very large, in order that the carbonic acid may bear a large proportion to other aeriform ſubſtances accidentally mixed with it.

Theſe are, doubtleſs, only a few of the phenomena, to the explanation of which your theory may be ſucceſsfully applied;

and I confidently expect that many facts, hitherto referred to chemical principles, will be brought, in conſequence of your diſcoveries, within the pale of mechanical philoſophy.*

I am, Dear Sir,
Your's very truly,
WILLIAM HENRY.

Mancheſter, June 20, 1804.

XIII.

On the Diſappearance of Oxigen and Hidrogen over Water, at the Heat of the Atmoſphere. By T. S. T.

To Mr. NICHOLSON.

SIR,

Slow abſorption of oxigen and hidrogen over water.

SOME months ago, I read in your excellent Journal an account of an experiment, which tended to ſhew that oxygenous and hydrogenous gaſes, when mixed together, and allowed to remain over the ſurface of water for a long time, ſpontaneouſly united and formed water.

Having long been accuſtomed to conſider a temperature conſiderably higher than that our atmoſphere ever attains, neceſſary to this union, I was naturally led to inveſtigate this phenomenon; and for that purpoſe undertook the following experiments:

1. I prepared oxygen gas from black oxide of mangaueſe, by means of concentrated ſulphuric acid, aided by heat, and in order to render it more pure, I waſhed it well with milk of lime. I prepared likewiſe a quantity of hydrogen gas, by paſſing a few drops of water through a gun-barrel, filled with iron filings, and paſſed through the body of a ſmall furnace, I introduced nearly equal quantities for both gaſes into a bell-glaſs jar, placed on the ſhelf of a common pneumatic trough, which ſtood in a room without fire, and almoſt without light. The mixture was ſuffered to remain in that ſituation for about five months; at the end of which time, the volume of the gaſes had diminiſhed $\frac{1}{3}$.

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* As the author had not ſeen Mr. Dalton's letter, publiſhed in our laſt number, at the time when theſe illuſtrations were written, he has mentioned a few circumſtances contained in the letter.

Slow abſorption of oxigen and hidrogen over water.

2. On reading the account above alluded to, it occurred to me that it was poſſible, that the diminution in bulk, might have ariſen from a partial abſorption of one or of both gaſes, by the water of the trough. In order to aſcertain this, I introduced like proportions of both gaſes, into a jar, placed in a mercurial trough, which was in the ſame room with the other; and after ſuffering this experiment to continue as long as the other, I found that of 12 cubic inches of both gaſes introduced into the jar, 3½ had diſappeared; but I could ſcarcely perceive any moiſture on the ſides of the jar, owing to the ſmall quantity of water which had been formed.

The decreaſe in volume, in theſe experiments, could not be owing to any condenſation of the gaſes, by the coolneſs of the ſurrounding air; for I found that it took place gradually; and the mixtures were made in the beginning of January, and ſtood till the end of May; conſequently there ſhould have been rather an increaſe than a decreaſe in bulk, if the temperature of the air was the cauſe.

The reſiduary air contained in the jars, ſtill conſiſted of oxygen and hydrogen gaſes; for when received into a phial, on the application of a lighted taper, a ſmart exploſion took place, and the ſides of the phial grew dim. On adding ſulphuret of lime to another portion of the reſidue, a rapid abſorption of the oxygen gas took place, and hydrogenous gas was left behind.

From theſe facts we may fairly conclude, that the decreaſe in volume was owing to the ſpontaneous combination of the two gaſes to form water.*

T. S. T.

Orkney, June 20th, 1804.

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* It deſerves to be conſidered whether the abſorption of the purer gaſes within, and the eſcape at the ſurface of the water expoſed to the atmoſphere, according to the doctrine explained in Mr. Henry's paper (page 297) may not have occaſioned the deficiency.

W. N.

XIV.

Letter from Dr. P. A. Nemnich, expreſſing Doubts with regard to the Death of the celebrated Humboldt.

To Mr. NICHOLSON.

SIR,

Hamburgh, June 29, 1804.

Inquiry concerning the death of Mr. Humboldt.

YOUR Journal of Natural Philoſophy, &c. June 1, 1804, page 72, mentions poſitively the death of Mr. Humboldt, which, as we had here in Germany no other advices, I immediately communicated to my countrymen through the channel of our newſpapers. There are however in Germany, as well as in France, many doubts about the validity of the ſaid notice, and many objections made. Having quoted your Journal, as above-mentioned, I ſhould be very much obliged to you for a more circumſtantial and poſitive account of this report of Mr. Humboldt's death, with the day of his deceaſe, and the way by which this notice reached England, &c. which as ſoon as I have received, I will inſtantly make public, in order to maintain that credibility your valuable Journal deſerves.

I am, Sir,
Your's moſt reſpectfully,
P. A. NEMNICH,
Licentiate.

Extrait du Publiciſte.

Paris, 20 Juin, 1804.

Extract from th Publiciſte.

VOTRE feuille de ce jour contient à l'article de Hambourg la nouvelle de la mort le Mr. de Humboldt. Il m'eſt permis d'en révoquer en doute l'authénticité, et de raſſurer les amis des ſciences et de l'humanité. Je ſais poſitivement que Mr. Guil. de Humboldt à Rome, a reçu de ſon frere des lettres datées de la Havane du 28 Mars, dans leſquelles il lui marquoit que ſous 12 jours il ſeroit rendu à Charleſtown d'ou il s'embarqueroit de ſuite pour le Havre, et qu'il comptait être à Paris avant la fin de ſuin.—La nouvelle eſt donc plus que douteuſe et nous pouvons eſperer, que le ſort ne ſe ſeroit pas fait un jeu cruel de rendre vain le dévouement ſans bornes et les nobles efforts de l' illuſtre voyageur. (Signé) Mendelſſohn.

Extract from the Publiciſte.

Paris, 20th June, 1804.

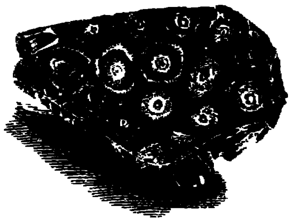
Tranſlation. YOUR number of this day contains under the article Hamburgh, an account of the death of Mr. de Humboldt. I have reaſons to doubt the authenticity of this article, and to encourage the friends of ſcience and of humanity. I know poſitively that Mr. William de Humboldt received from his brother letters dated from the Havannah, of the 28th March, in which he informs him that in twelve days he ſhould go to Charleſtown and embark for Havre, with the expectation of arriving in Paris before the end of June. Your article of news is therefore more than doubtful, and we may hope that the courſe of events have not been ſo unfavourable as to render the unlimited ſacrifices and efforts of this illuſtrious traveller of no uſe to ſociety.

⁂ Dr. Gibbes, the author of the note in queſtion, will, no doubt, have the goodneſs to mention his authority, when he ſees this. W. N.

☞ *From the extraordinary Number of valuable Communications this Month, (every one of the Articles, but thoſe of* GAY LUSSAC *and* BARTHOLDI, *being original) it has been neceſſary to poſtpone an excellent Memoir on Hauy's Syſtem, by the Abbé Briel, and a Paper by* E. O. *on the Computation of Tables of Squares and Cubes, both which, and ſome Abridgements and Collections of intereſting Matter from the Phil. Tranſactions, together with the Scientific News, will appear in our next.*

· saique painting composed in Glaſs.

Fig. 1



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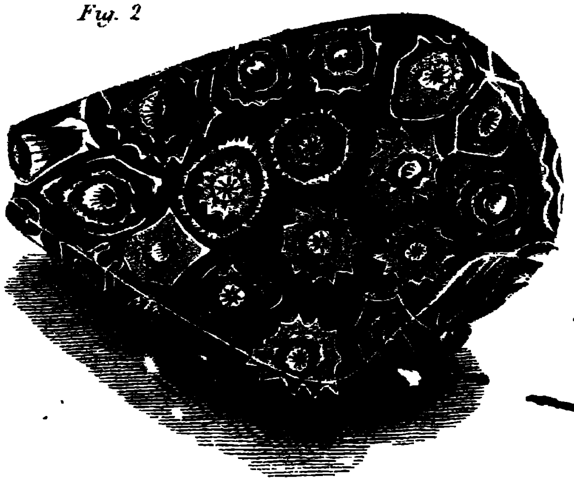

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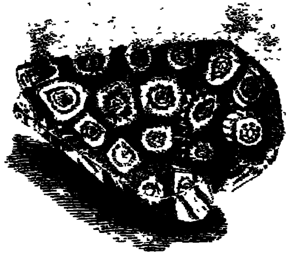
Philos. Journal. Vol. VIII. Pl. XIII. p.304

Antique painting composed in Glass.

Fig. 1



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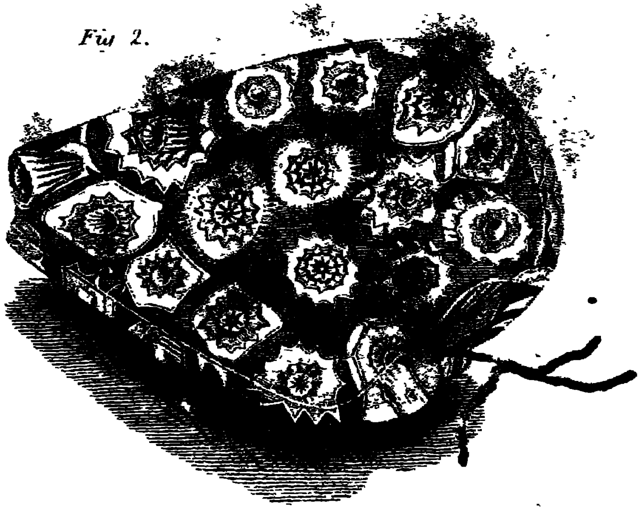
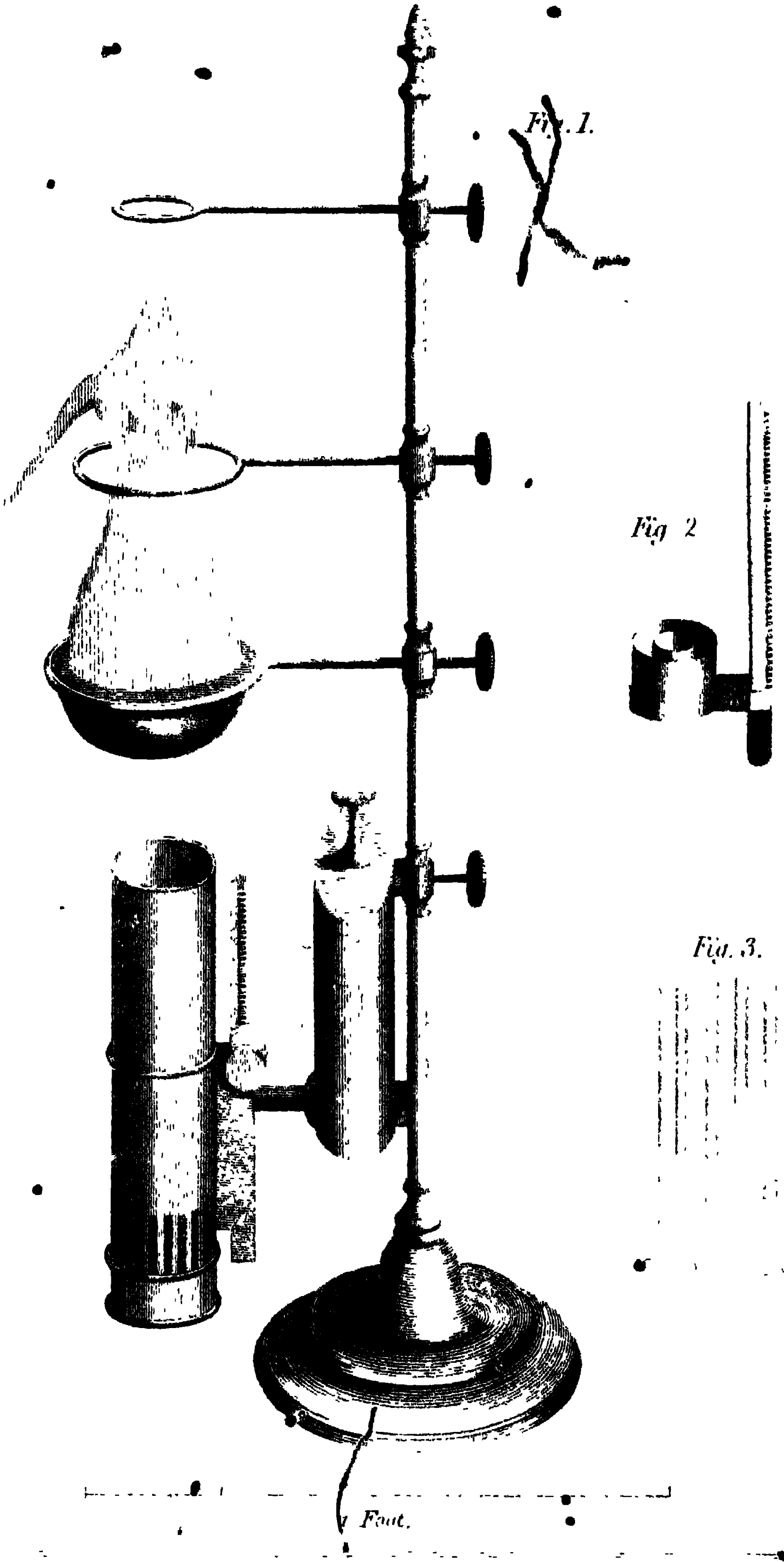


Mutlow Sc. Russell Ct.

Improved Chemical Lamp.



Drawn by Blunt

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